

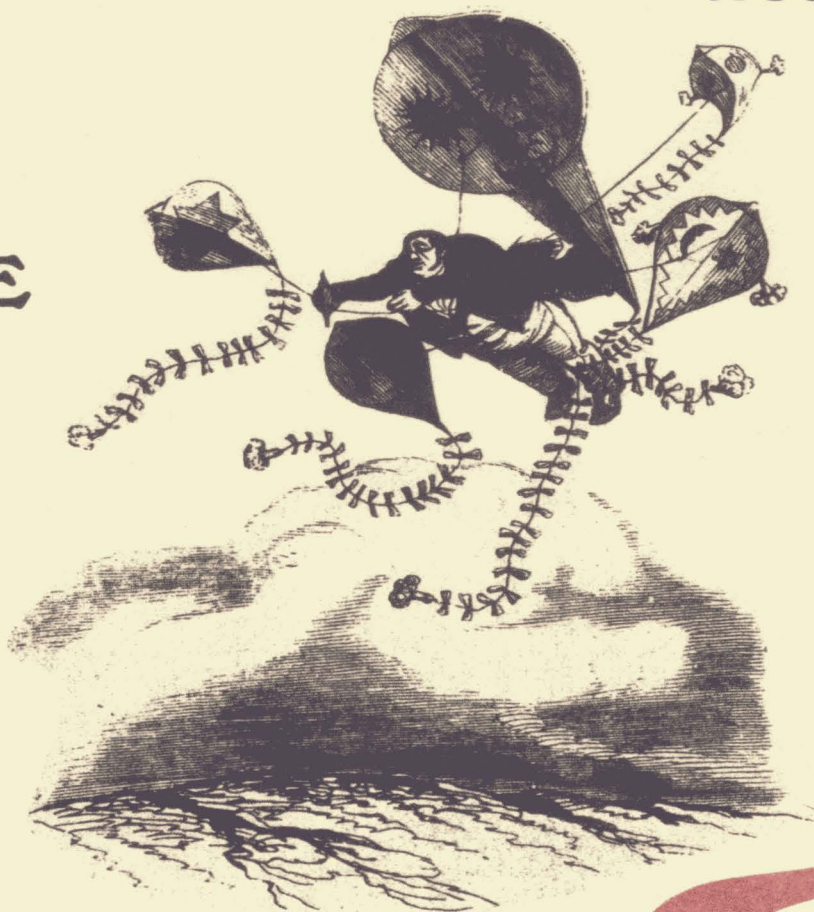
Zetetic scholar

An Independent Scientific Review of Claims of Anomalies and the Paranormal

No. 7

1980

**BIG
DIALOGUE
ON
UFOS
&
MORE!**



Zetetic scholar

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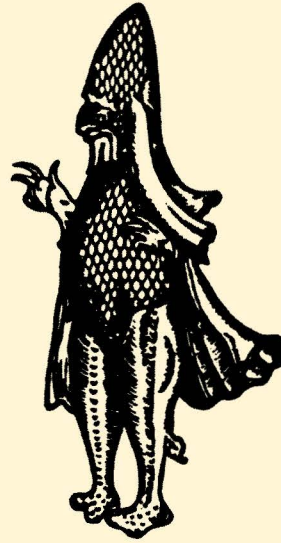
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AN INDEPENDENT SCIENTIFIC REVIEW OF
CLAIMS OF ANOMALIES AND THE PARANORMAL



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* *New appointments.*

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CONTENTS

NUMBER 7

DECEMBER 1980

ARTICLES

- I.J. GOOD
Scientific Speculations on the Paranormal and the
Parasciences.....9
- RICHARD H. HALL
Livestock Mutilations: A National Mystery.....43

SPECIAL ZS BIBLIOGRAPHIC FEATURES

- HENRY H. BAUER
The Loch Ness Monster: A Guide to the Literature.....30
- GEORGE P. HANSEN
Bibliography of Bibliographies on Dowsing.....101

NEW ZS DIALOGUE

- J. RICHARD GREENWELL
Theories, Hypotheses, and Speculation on the Origins of UFOs...52

Critical Comments BY:

- | | |
|----------------------------|--------------------------|
| GEORGE O. ABELL.....60 | J. ALLEN HYNEK.....84 |
| JEROME CLARK.....67 | JOHN A. KEEL.....85 |
| DANIEL COHEN.....69 | BRUCE MACCABEE.....86 |
| WILLIAM R. CORLISS.....71 | AIMÉ MICHEL.....88 |
| JOHN S. DERR.....72 | JAMES W. MOSLEY.....89 |
| CHARLES FAIR.....73 | JAMES E. OBERG.....89 |
| ROBERTO FARABONE.....75 | JOHN RIMMER.....90 |
| LUCIUS FARISH.....76 | MICHAEL K. SCHUTZ.....92 |
| STANTON T. FRIEDMAN.....77 | ROBERT SHEAFFER.....94 |
| ALLAN HENDRY.....79 | P.A. STURROCK.....96 |
| ELAINE HENDRY.....80 | DAVID W. SWIFT.....99 |
| RICHARD C. HENRY.....83 | |

CONTINUING ZS DIALOGUES

- RAY HYMAN
Reply to the Commentators on "Pathological Science".....113
- JOSEPH MAY replies to Geoffrey Dean re Velikovsky.....121
- LAURENT BEAUREGARD comments on Ray Hyman's "Pathological Science".122
- RAY HYMAN replies to Laurent Beauregard.....123
- BRADLEY DOWDEN comments on Robert G. Jahn's "Psychic Research"...125
- ROBERT G. JAHN replies to Bradley Dowden.....127

Contents continued.

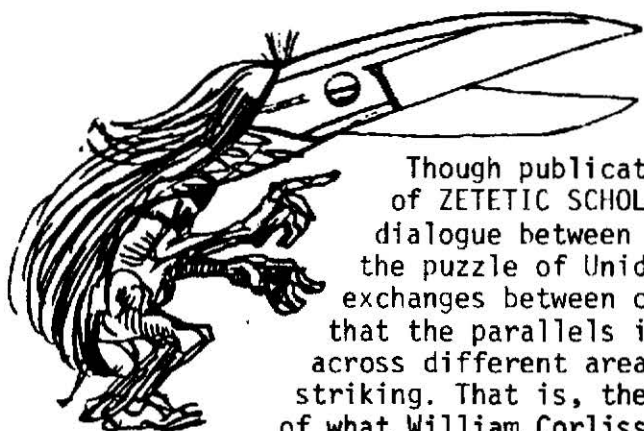
DAVID MORRISON replies to Joseph May, Leroy Ellenberger, and C.J. Ransom re Velikovsky's theories.....	128
SOLOMON E. FELDMAN comments on Edward W. Karnes, <u>et al.</u> , re "Remote Viewing".....	131
PIET HEIN HOEBENS cooments on Edward W. Karbes, <u>et al.</u> , re "Remote Viewing".....	131
EVAN HARRIS WALKER comments on Edward W. Karnes, <u>et al.</u> , re "Remote Viewing".....	132
EDWARD W. KARNES replies to Solomon E. Feldman, Piet Hein Hoebens, and Evan Harris Walker.....	137
CHRISTOPHER SCOTT replies to John Beloff and Sybo Schouten re "Seven Evidential Experiments" for psi.....	140
JOHN BELOFF replies to Christopher Scott.....	144
JOSEPH AGASSI replies to Andreas N. Maris Van Blaaderen re "Superstition".....	145
GEOFFREY DEAN adds to Andreas N. Maris Van Blaaderen's remarks on Astrology.....	146
JON BECKJORD replies to Robin Ridington re Sasquatch "Photos".....	148

REGULAR FEATURES

EDITORIAL.....	3
LETTERS: SHERMAN STEIN & HOWARD WEINER - C.E. HANSEL - RICHARD DE MILLE - PETER ROGERSON - I.J. GOOD.....	4
RANDOM BIBLIOGRAPHY ON THE OCCULT AND THE PARANORMAL.....	106
SUPPLEMENTS TO PAST ZS BIBLIOGRAPHIES.....	112
BOOK REVIEWS	
Lutz Müller's <i>Para Psi und Pseudo: Parapsychologie und die Wissenschaft von der Täuschung</i> (PIET HEIN HOEBENS).....	150
Jacques Vallee's <i>Messengers of Deception: UFO Contacts and Cults</i> (J. RICHARD GREENWELL).....	154
Ronald Story's <i>Guardians of the Universe?</i> (MORRIS GORAN).....	157
BOOKS BRIEFLY NOTED.....	159
ERRATA.....	4
ABOUT THE CONTRIBUTORS TO THIS ISSUE.....	164

JAMES N.C. WEBB (1946-1980)

James Webb, a Scottish writer and consulting editor to ZETETIC SCHOLAR died suddenly in Dumfriesshire, Scotland, on May 9th. He was a fine scholar and human being and will be greatly missed. His education at Harrow School and Trinity College, Cambridge, was followed by a brief career in teaching and broadcasting after which he became a full time writer. His books THE FLIGHT FROM REASON, THE OCCULT ESTABLISHMENT, and THE HARMONIOUS CIRCLE (a definitive study of Gurdjieff and Ouspensky) established him as a leading expert on the occult., magic, mysticism and general history. All of us interested in these areas remain in his debt. We all extend our sympathy to his widow, Mary.



EDITORIAL

Though publication has been slightly delayed, this issue of ZETETIC SCHOLAR continues the spirit of responsible dialogue between proponents and critics, extended now to the puzzle of Unidentified Flying Objects. As one reads the exchanges between commentators, it should become rather apparent that the parallels in argument between proponents and critics across different areas of claims of anomalous phenomena are quite striking. That is, the arguments put forward about various parts of what William Corliss calls the "unclassified residuum" of science are similar. First, the matter of whether or not an anomaly is real or merely alleged must be determined. This is usually in part a function not only of direct evidence but also of the theoretical context (usually involving proposed mechanisms) which makes the alleged anomaly appear more or less plausible. Thus, theory and evidence are usually intertwined. This is far less so for what I have called the cryptosciences (which merely claim an extraordinary variable's existence rather than an inferred relationship between variables as in the parasciences; thus in the cryptosciences it is hypothetically possible to prove existence of the anomaly by "bringing in the body," as in cryptozoology). But in most cases, the proposed anomaly is by definition theoretically unhoused. Thus, the ultimate explanation for even an accepted anomaly may take place rather far afield from initial expectation of its proponents. Thus, if psi is real, its explanation may result in our revamping our ideas about statistics rather than perceptual psychology. Or if UFOs are real, they may ultimately be explained by the meteorologists or psychologists rather than the exobiologists. Whatever may account for the reports of UFOs may also, in part at least, account for reports of apparitions like phantom ships or even fairies.

The important thing, from the ZS point of view, is to develop constructive rather than destructive ways of scientifically dealing with extraordinary claims. On the one hand, this involves a heuristic approach of "agnosticism" towards at least the initial claims. On the other hand, our concern is with examining claims in order to advance science wherever that might lead. It might lead to validation of the claims. But more than likely it will lead to falsifications of the extreme versions of the claims. Yet even if all we learn is that error has been committed, by carefully examining such error, we may at least learn something valuable about how such errors are made. The object of science is to expand our knowledge and not merely to use our existing knowledge to discredit or "explain away" new claims. Instead of viewing a discrediting explanation of an anomaly as a disappointment, we should view it as an advance that may help us to expand our scientific vision. Most "normal" explanations of paranormal phenomena are seen as "answers" when they usually open new questions. For example, the plasma explanation of UFOs (as proposed by P.J. Klass) --if true-- may disappoint those who wished for an extraterrestrial explanation, but it recasts the anomaly into a problem (perhaps an even bigger theoretical anomaly) for those in meteorology. In similar fashion, the "Clever Hans" explanation for alleged communication between people and apes actually represents a serious problem for those concerned with nonverbal communication. ZS therefore argues for an interdisciplinary and constructive approach. Valid anomalies are forces for growth and progress; we should welcome, not fear them.

ERRATA

In the last issue of ZETETIC SCHOLAR (#6), in the article "Scientists and Anomalous Phenomena: Preliminary Results of a Survey," by Greenwell and King, two sentences were inadvertently omitted. On page 20, paragraph 3, the sentence should have read: "Among all three groups, 56.4% favor Bigfoot research (physical anthropologists 61.5%, physical chemists 42.4%, marine biologists 53.2%), and 64.3% favor Nessie research (physical anthropologists 70%, physical chemists 56.5%, marine biologists 64.5%)." And on page 21, paragraph 2, the sentence should have read: "We find that 71.8% of the physical anthropologists identified themselves when responding on Bigfoot, but only 33.3% did so when responding on Nessie."

LETTERS

In his letter of January 9, 1980, Dr. Tart refers to our "absolutist" way of thinking about randomness. We should explain what "absolutist" means in this context. Since his first experiment had flaws that could be avoided by use of a better design, and since it was not necessary to travel to the moon to do a second experiment, we urged Dr. Tart to repeat the experiment, incorporating a reliable source of random numbers.

We also urged him to adopt a double blind approach. (At the minimum, the person recording the sequence of guesses should not know the target sequence.) This he did not do. Until the experiment is conducted properly, we agree with Richard Hamming, the information theorist, that there is no point in poring over the data in a search for hidden patterns.

We would be delighted if ESP were some day proved to exist. We have accepted more astonishing phenomena, such as the action of gravity and a magnetic field at a distance.

But anxious as we are to believe, we will not let ourselves be persuaded by unconvincing experiments.

-- SHERMAN STEIN & HOWARD WEINER

In the Zetetic Scholar No. 6, 1980, under "Books Briefly Noted," the note on my book "ESP and Parapsychology: a critical reevaluation," contains the statement "Unfortunately, the new edition suffers from the same problems as the old one with Hansel taking little note of the many criticisms made." What are these problems? Are your readers to rely on the value judgements of an anonymous reviewer? Could these problems be listed--or if there are too many of them, perhaps the half dozen more important ones could be given?

Your reviewer also mentions that the new material in my book is open to counter criticisms. Surely it is up to him to state these counter criticisms or at least give some idea of what they amount to.

-- C.E.M. HANSEL

M. TRUZZI REPLIES:

The "Books Briefly Noted" section is all written by the Editor and my initials appear at the end of the section, so the reviewer is not anonymous; but starting with this issue my authorship will be indicated more prominently to avoid future confusion. These "brief notes" are meant to give my general reaction to the works covered --mainly to call them to the attention of ZS readers-- and are not intended to be substitutes for full and detailed reviews (which often follow in a later issue). Professor Hansel's earlier edition of his book was widely reviewed in the parapsychological press (e.g., Medhurst's review in the JSPR, Stevenson's review in the JASPR, Honorton's review in the J. of Parapsychology, to say nothing of reactions to and exchanges on his book by R.A. McConnell (in Contemporary Psychology) and H.J. Eysenck, et al. (in the British J. of Psychiatry). All these reviewers raised issues that are neither directly answered or corrected in the new edition of Hansel's book. The burden is not upon me--especially in a brief book note--to itemize the many issues raised. The burden is upon Professor Hansel to be familiar with the reviews of his book in the standard journals where he might have expected replies and analysis. In addition, major criticisms have been raised by Michael Martin (J. of Parapsychology, June 1979) and Trevor Pinch (Social Studies of Science, 9, 1979, 329-348) in recent years. It would appear that Hansel ignored these criticisms since he neither cites them or responds to them. As for the counter criticisms of the new material, much of that falls along similar lines (e.g., the problem of falsifiability of Hansel's doctrine that the possibility of fraud alone invalidates claims. However, there are additional specific difficulties which I have confidence will appear in the reviews of his new edition (such as the review by Sargent in ALPHA, by Rogo in Fate, and Palmer in a forthcoming issue of Contemporary Psychology). I will not itemize my own new reactions here, but I may do so in a future article. There seems little point in presenting Professor Hansel with new counter arguments when he seems to have thus far ignored most of the ones already available for him.

Paul Thagard's comment on Ray Hyman's dialogue-opener contains a pair of intriguing, debatable assumptions: that people who believe they are guiding their lives with the help of astrology make their life choices (1) differently from and (2) worse than people who depend on philosophy, psychology, medicine, or some other repository of knowledge. I think it would be difficult to adduce evidence in support of these assumptions. The idea that the personal lives of modern, scientifically-oriented people are happier, saner, healthier, wealthier, longer, or wiser than the lives of old-fashioned, superstitious people is an armchair opinion. The idea that people are actually guided by what they claim they are guided by is highly suspect. In consequence, the Humanist (if you will pardon the oxymoron) crusade against superstition may be a hunt for dragons.

-- RICHARD DE MILLE

I was pleased to read the excellent dialogue between Ron Westrum and Allan Hendry in issue #5 of ZS. Both authors made some exceedingly valid points, though, on balance, I tend to side with Hendry on most points.

Westrum's quote of Hendry (Hendry 1979, p. 157) as saying that UFO hoaxers "feel the need to create an external, concrete expression of the same, emotional, ufological turmoil which they don't understand themselves" is taken rather out of context and gives the impression that this is some definite conclusion that Hendry has reached and which Westrum proceeds to criticize as "vague." However, the context of the original quote reads: "Why should some people be compelled to proffer hoaxed UFO photos as genuine, at the risk of exposure and extreme ridicule?...If UFO visions are the result of extraordinary cultural pressures in search of self-expression, then hoaxers, like IFO witnesses, are simply attempting the same thing, but feel the need to create an external, concrete expression of the same inner, emotional ufological turmoil which they probably don't understand themselves" (Hendry 1979, p. 157). This context clearly establishes that it is part of Hendry's review of the works of John Rimmer (1978) and myself as published in MUFOB (now MAGONIA). I think it is clear that Hendry was presenting these views as an interesting line of thought and in no way totally committing himself personally to them.

In our articles, we were discussing the whole range of hoaxes, not just photos. One argument which we floated was that the existence of photos and physical traces allowed the witness to assure him/herself of the experience.

Perhaps we can go some way towards determining whether UFO photo hoaxes represent an attempt to "fix" UFO experiences in "reality" or whether they are "nothing but" a joke by comparing the accounts provided by UFO photo hoaxers with those provided by "genuine" UFO and IFO percipients.

I tend to agree with Westrum that the source of UFO imagery probably lies in cultural symbols, the origin of which remains an intriguing historical puzzle. We should realize that there were times before 1947 when people reported stars and planets as strange and ominous machines, to wit, the airship panics of 1897, 1909, and 1913, during which people looked at stars and "saw" airships. The 1913 panic (Sandell 1978) was, fortunately, received both contemporary (Hirst 1913) and subsequent (Dangerfield 1970) historical treatment. We now know that both the 1909 and 1913 airship waves were part of a larger spy scare (Watson 1980) which produced many of the motifs that were to occur later in UFO stories (Men-In-Black, ambiguous physical evidence, pilots interested in poultry, absurd conversations, etc.).

Perhaps the reason why people get mad, when a prosaic solution is offered for their UFO experiences, is that the ufologists have done a marvelous propaganda job in persuading people that any explanation of a UFO experience which does not invoke extra-human intelligences may be a dastardly "explaining away" by a cruel hearted cynic who is probably in the pay of either the authorities or some nefarious conspiracy.

Finally, Ron Westrum rightly raises the question of the bounds of contemporary folklore. The UFOfolklore is increasingly merging with that of ancient astronauts, the Bermuda Triangle, bigfoot,² cattle mutilations, poltergeists, assassination conspiracies, etc. Attempts to define a global symbolism for this folklore do not seem very promising, so perhaps we ought to address ourselves to the meaning that this folklore has for various discreet social groups and individuals.

NOTES:

¹MAGONIA. Editor: John Rimmer. 64 Alric Avenue, New Malden, Surrey KT3 4JW, England.

²Ridington (1979) strikes me as an excellent study of the bigfoot symbolism and is very relevant to the UFO folklore.

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-- PETER ROGERSON



I have two comments concerning the article by Greenwell and King (Zetetic Scholar, No. 6, 1980) on Bigfoot and Nessie: (i) Without a copy of their questionnaires the reader cannot judge whether some of the respondents were justified in being somewhat critical of it. (ii) On page 29, in their summary they say

"If there is a 'proper' scientific attitude, one would expect the responses of all three groups to be very much the same. As in many instances they were not, the respondents must have been influenced by other, non-empirical factors. We will not at this time propose what these other factors may be, other than to state that they necessarily must fall within the realm of psychology. When further statistical analyses are completed, we may be able to shed more light on this interesting social phenomenon."

While no doubt the psychology of the respondents is relevant, perhaps even more relevant is what they happen to have read on the topics, and how much they have thought about them. Of course the amount of reading and thinking depends partly on psychology, but the psychological aspects would be clearer if all respondents had first listened to debates concerning the existence or nonexistence of the creatures in question.

-- I.J.GOOD



A PLEA FROM THE EDITOR:

The future of ZETETIC SCHOLAR remains problematic. Thus far, ZS has not increased its circulation to the point that it can cover its costs. ZS will continue publication, but it seems unlikely that future issues can remain as large as they have been. Reduction in print size may be a partial solution. Let me know your thoughts. If ZS can obtain a full 600 regular subscribers, its future will be assured. We could increase subscriptions by popularization, but this would defeat the purpose of ZS as a serious responsible, and scholarly publication. Another alternative is to raise the price for ZS even higher, but that, too, would be self defeating. Your suggestions, ideas and help would be most welcome. This "agnostic" journal needs an "angel."

SCIENTIFIC SPECULATIONS ON THE PARANORMAL AND THE PARASCIENCES

I.J. GOOD

If ESP is possible it might manifest itself either spontaneously or non-spontaneously. The non-spontaneous kind can be tested experimentally. Some of the most notorious experiments were performed by S.G. Soal in England in the 1930's and 1940's. I call them notorious instead of famous because there is now overwhelming evidence that they were fraudulent. (See, for example, Hansel, 1980, Chapter 12.) In 1974 I judged the betting probability of non-spontaneous precognitive telepathy at about 1/5. I would now give odds of 100 to 1 against non-spontaneous telepathy. It would be easier to try to carry out an experiment that would convince me of genuine strong clairvoyance in half-an-hour, and I would bet \$10,000 to \$100 against it, both prizes to go to charities selected by me. Experiments in all kinds of ESP have been going on for a long time without convincing results repeatable by numerous independent experimenters, and the longer this goes on the smaller the probability becomes. Arguments against the validity of some of the well known apparently successful experiments in parapsychology can be found also in Marks & Kamman (1980), and in the journal The Skeptical Inquirer. But The Zetetic Scholar is somewhat more

*This paper was presented at the tenth annual meeting of the Popular Culture Association, held jointly with the second annual meeting of the American Culture Association at Detroit, Michigan, 1980, April 16-19. Session on the Philosophy of Parapsychology, 8.30-10.00 a.m. April 19, at the Book Cadillac Hotel. The session was in the Crystal Ballroom, appropriately enough!

** Reduced to \$5000 after seeing Targ & Puthoff (1977)

sympathetic to the paranormal although its first issue (1978) contains a list of 56 books that appear to debunk the paranormal.

If someone takes up my challenge I might insist that conjurers, psychologists, and a movie camera should be present to detect cheating, and the loser must pay everybody's expenses. If the alleged psychic is caught cheating he will be fined \$50,000.

Many psychologists make use of "experimental dissimulation," in which they deliberately mislead their subjects. In other words, they are self-confessed liars. Set a thief to catch a thief; set a psychologist to catch a parapsychologist.

Parapsychologists often have strong incentives to cheat. For example, S.G. Soal presumably would not have been awarded a doctorate if his results had not appeared to be successful. It has been alleged that Carlos Castaneda also obtained his doctorate by fraud (de Mille, 1976), and he started a quasireligion that hasn't died yet.

In short, I think John Wheeler (1979) was probably right to say "Where there's smoke there's smoke." But I don't think parapsychology should be dismissed out of hand because it is important if there happens to be anything in it.

The failures of the past rationally decrease the subjective probability of ESP, of the non-spontaneous kind, as I said; but the probability does not tend to zero; it must tend to some non-zero limit. This can be seen by the following argument.

Let a somewhat arbitrary sharp definition be laid down for whether a person can readily exhibit ESP. The specific definition is not important for my argument. Let p_n denote the prior probability that the fraction of the population who can readily exhibit ESP is in the interval $(10^{-n-1}, 10^{-n})$ ($n=0,1,2,\dots$). The evidence to-date might rationally, by Bayesian arguments, decrease p_n by a large factor for $n \leq 4$, but it has little bearing on the values of p_n for $n \geq 9$ because the one-in-a-billion percipients might be obscure Yogis who have religious reasons for reticence about their abilities. Perhaps I should add that J.B. Rhine thought about a fifth of the population can exhibit ESP, so I suppose he would not have agreed that even p_0 has been diminished.

With regard to spontaneous, non-experimental, ESP, there is a simple argument, usually overlooked, that should prevent us from believing too readily. I shall quote from Good (1966):

If some one reports a dream, which is written down and witnessed by reputable witnesses, and which turns out to be apparently precognitive, we have to judge a number of quantitative matters: (i) how improbable was the apparent coincidence, say probability p ; (ii) how many events occurred in the man's dreams, say d , holding in mind that most of us dream far more than we [used to] think; (iii) how many

events occur in waking life, say w ; (iv) from how large a group was the person selected, say g . Then we must judge whether the product dwg is much less than $1/p$. Now we do not know how to do all this, but this is what we must informally be trying to do when we judge that the dream was genuinely precognitive. As an example, d might be 100,000, w might be 1,000,000 and g might be 1,000,000; and in this case we need p to be much smaller than 0.000000000000000000. These figures are only illustrative, and the theory used here is half-baked, but they might act as a warning against gullibility.

This argument does not even allow for the possibility of fraud nor for exaggerations and tricks of memory, and wishful thinking. If there were a newspaper that published dreams, as was suggested in 1975 and probably earlier, one could see if some of the dreams predicted events, not under human control, that occurred after the dreams were published. Then fraud etc. could be ruled out. In this way spontaneous precognition could be firmly established once and for all, if good enough cases occur. The people who provide funds for parapsychology should hold this point in mind before they allot large funds to experimenters who allow themselves to be tricked by "magicians." It should not be forgotten, however, that it is usually difficult to measure the magnitude of a coincidence.

I do not know whether there is any known case of spontaneous ESP that can overcome thoroughly stringent criteria. And if there have been lots of coincidences of the order 10^{-18} , why haven't there been some much more decisive ones of order such as 10^{-50} ? If there is good evidence, why isn't there some overwhelming evidence also? Could it be that paranormal phenomena are miracles intended by some power to encourage us to become religious, and which reveal themselves only "through a glass darkly" or are "shy" as it is sometimes expressed? It is a well known theological thesis that one's faith is not supposed to be forced upon one. That is, one's subjective probability of the truth of the religion must not be forcibly increased much. If this is so, a miracle can occur only to people whose degree of belief is already close to 1. When, on rare occasions, miracles are presented to people whose degrees of belief are not close to 1, they will suspect trickery so they will not be forced into complete belief. It is certainly rational to suspect trickery in this imperfect world. Jim Jones performed fraudulent miracles, so perhaps other religious leaders did so too.

If paranormal phenomena are always shy, then it will never be possible to obtain good scientific evidence for them; it would be mainly a matter of faith, the faith that gives comfort, and which is exploited for financial gain and power by charlatans or fanatics, like Jim Jones, Ron Hubbard and the Reverend Moon, and leads to holy wars, witch-hunting, the doing unto others as you would not wish to be done by, the torturing of people for the good of their souls or because they are assumed not to have souls (being of a wrong color), and, as in Irish Christianity, the turning of the other man's other cheek.

If the paranormal belongs only to the realm of religion, then the observations, both for isolated results, and for the entire body of the evidence, will continue to be suggestive but never conclusive.

Perhaps every quasiscience is a quasireligion, and parapsychology might be both.

In case you think my references to the "glass darkly" are tongue-in-cheek I must say that "coincidences" in my own life have prevented me from being in a state of total disbelief. For example, one day in 1956, I became interested in a theorem in statistical theory, for which a colleague had produced a long proof. A day or two later, on my first visit to the mathematics library in UCLA, immediately upon entering, I took a periodical entirely at random off the shelves and opened it at random. The two pages thus revealed contained a purely mathematical theorem which I at once recognized solved the statistics problem (see Dawson & Good, 1957). If it had not been for this event I would now perhaps still have been a British Civil Servant, but I won't bore you with the reasons for that. Allowing for my guess (10^{-4}) at the prior logical probability that such a theorem was in the literature at all, I would put the probability of the event at about 10^{-11} . What's more, although this might sound like Al Franken on "Saturday Night Live," it happened to me personally, not just to some random person in the world. This kind of direct experience carries a lot of weight and prevents me from being dogmatically against religion and quasireligion.

It is sometimes claimed that telepathy, clairvoyance, and precognition are apt to occur in situations of great emotional significance. It is difficult or expensive or unethical to bring such situations into a controlled experiment. It might be possible by offering potential percipients immense rewards such as reprieves to prisoners on Death Row. Or, more unethically, immense punishment could be given for failure, as by kings in fairy stories. Idi Amin could have organized such experiments if he had been interested in the scientific study of the paranormal.

The parapsychological Powers That Be might disapprove of such experiments. Since it is difficult for the experimental evidence alone to prove or to dispose of parapsychology, it is of some interest to consider whether modern science and rationality can be brought to bear on its prior probability, that is, on the probability before one examines the direct experimental and observational evidence.

One very strong argument against ESP is that, since it would have great survival value for the individual, the ability should have been improved by Natural Selection. So I cannot understand why most of us don't have marked ESP powers if such powers are physically possible. Against this argument James S. Hayes, a psychiatrist, suggested in 1951 that we do have good telepathic powers, but we suppress them presumably to avoid being overwhelmed by too much information. My response is that if we do have good ESP powers we would use them selectively to good advantage, just as we use our other senses. Why pick on ESP for suppression? (See also Appendix E.)

At first blush, the most obvious physical hypothesis to explain

telepathy, if it is possible, is that it operates by electromagnetic radiation. The brain gives out a little radiation that can be picked up by the electroencephalograph. Moreover some of our senses are exceedingly sensitive. The eye can detect a few photons of light under the right circumstances, the ear can detect vibrations of amplitude the diameter of an atom, and a dog can chase a man by smelling the ground he has walked on. Maybe the billions of slinky-like chromosomes in a quiescent brain can resonate to extraordinarily small fields of energy and can reinforce one another (Good, 1967, p.21). Against this suggestion is the claim, often made, that telepathy can operate at great distances, but then so can radio. According to the calculations of J.G. Taylor and Balanovski (1979), electromagnetic radiation cannot be invoked to explain telepathy; and they regard this as "a strong argument against the validity of the paranormal." I don't know whether they are right. It should also be recalled that Russian parapsychologists claim to have achieved successes in telepathic hypnotism with electromagnetic fields shielded off.

The telepathic field might be something new to physics, or, as I conjectured in 1946 or 1947, it might be the Schrödinger wave function, which is supposed to be sufficient in principle to explain the whole of chemistry, and which happens to be denoted by the Greek letter psi. (Good, 1962a.) This conjecture is somewhat strengthened by the paradox of Einstein, Podolsky and Rosen (1935) in which two particles, having been once together, have to be regarded as part of the same quantum-mechanical system even when they have become separated by a great distance. Let me quote from Zweifel (1974):

...if photon A is in a state of right (resp. left) circular polarization, the photon B is always also in a state of right (resp. left) circular polarization; on the other hand, if the linear polarization of photon A is measured instead, then photon B is always in the same definite state of linear polarization. As Kasday ... puts it, the photon 'decided in advance' the type of measurement which was to be performed on A, and adjusted its own state accordingly.

Zweifel, while remaining non-committal about the explanation, shows how the paradox can be avoided by invoking the consciousness of the experimenter in accordance with a proposal due to Eugene Wigner. For more of my comments on the EPR paradox see Zweifel (1974, p.71n) and Good (1963).

The possibility of a telepathic field of a new kind became somewhat more probable, in my judgement, when physicists recognized new fields of force, the strong and weak interactions of "elementary" particles," in addition to electromagnetic and gravitational fields. For if there are as many as four distinct physical fields of force, it is not very ad hoc to assume a fifth one connected with consciousness, especially if this fifth field is the wave function (which, however, requires 3N dimensions for its description, where N is very large in the present application). This argument was weakened somewhat when Stephen Weinberg and Salam were promoted to the Nobility for apparently showing how the weak interactions and the electromagnetic field could be regarded as aspects of the same field. [I confess that

I find Weinberg (1967) unintelligible as also do many professional physicists.] But, if there are three fields, the adhocery of assuming a fourth is still not great, especially since science does not have a non-behavioral explanation for consciousness.

Laplace thought that the positions and velocities of all the particles in the world could determine the future uniquely. He overlooked that he was not in a position to explain the simplest chemical reaction. A modern psychologist who is confident that we already know enough in principle to explain consciousness, and who regards the assumption of "fields" of consciousness as ad hoc, is perhaps as dogmatic as Laplace was.

Consciousness has always seemed to many of us to be a great mystery. The notion that consciousness is only an aspect of behavior seems absurd to me. I don't believe that a cog-wheel machine could have more than a negligible amount of consciousness. It might be argued that machines can see with photo-electric cells, and can simulate much other behavior; but if we say a machine or a person can actually feel pain, then it seems to me that we are not just talking about behavior (compare Good, 1962b). There's a trite limerick on this topic:

There was a faith-healer of Deal
Who said "Although pain isn't real
When I sit on a pin
And it punctures my skin
I dislike what I fancy I feel.

I might have substituted "materialist" for "faith-healer" but it wouldn't scan. Beta-endorphin, which is produced by the pituitary gland, diminishes pain but does not diminish its mystery.

It is the mystery of consciousness more than anything else that to me puts a lower bound on the probability of ESP and on the existence of some kind of God. I think it is today by far the strongest argument for a belief in religion, but there are strong enough arguments against, God knows.

In the remainder of this article I shall bring together some further speculations about the religious and parascientific indications of science. They are based in part on my article "And Good saw that it was God(d)," Good (1975/1979) which contains earlier references. I have broken the material into a main text and some appendices, but the appendices are meant to be read and they are separated off merely for convenience of exposition.

H.L. Mencken said "Science, at bottom, is really anti-intellectual. It always distrusts pure reason, and demands the production of objective fact." (Quoted in Cohen & Cohen, 1971.) Instead of being scientific in this sense, the remainder of this article is speculative. As someone once said, speculation is another name for thinking. Although for me the probability of the existence of the paranormal is small, its importance if it exists is large enough to justify some speculation. This justification for looking for an explanation of what might not exist is like Blaise Pascal's argument for religious faith. (See, for example, Hick, 1967, p.166.)

Historically, science and religion have been antagonistic because the high priests used to make incorrect scientific assertions by "common sense" or by "divine revelation" and because they were powerful and aggressive enough to punish those who disagreed with them. A more modern and fairly popular tenet is that science and religion are independent of each other, that neither can say anything useful about the other. But my theme now is that there are some indications in science that there might be something in religion. This is more obviously so if "science" included "parascience." For if telepathy is possible, then it is plausible that we are all a part of a single consciousness, and that seems to me to be a religious concept. This concept is at least reminiscent of Jung's notion of the Collective Unconscious, but I hesitate to cite Jung's scientific authority in such matters. For Jung appeared to believe in astrology, yet admitted to Edgar Wind that he only pretended to do so because it helped his patients and himself. (See Appendix A.)

When I say there are religious indications in science, I do not mean there are any proofs, and the indications would not be accepted by most scientists. I hope that in what follows it will be clear what is intended to be only speculation.

It is commonly stated by physicists that, in accordance with the Special Theory of Relativity, no signal can travel faster than light, 3×10^{10} cm/sec. But it would be more accurate to say that if such a signal is possible then, according to some other observers, the signal would have infinite speed and according to others it would travel backwards in time (for example, Good, 1962, 1966). In the theory of black holes, which depends on General Relativity, time gets interchanged with one spacial dimension, thus making time travel apparently possible in principle though I don't understand the theory well enough to be sure of this. Then again, there is a familiar concept due to Feynman that a positron can be regarded as an electron moving backwards in time, but most physicists do not believe that time travel could be possible, partly because it would apparently lead to paradoxes; for a knowledge of the future would enable you to change it, so where did the signal come from? There is, however, a "branching universe" theory in which these paradoxes can be resolved (see Appendix C). This resolution is not a part of the Everett-Wheeler-Graham theory, but is an earlier science fiction idea also mentioned in Appendix C.

Thus it seems that the theory of relativity does not make it logically impossible for signals or particles to travel faster than light, though it apparently decreases the logical probability that this is possible. Such particles have even been named ("Tachyons") although their existence is not yet accepted. The detection of such particles was claimed in March 1974 by two young physicists in the University of Adelaide, but the results have not been confirmed and maybe these physicists just happened to be up for tenure at the time.

The obvious implication from the possible existence of tachyons is that precognition would be consistent with at least some respectable theories in physics. A more specific way of making this point is in terms of the "whispering gallery theory" of precognition (Good, 1962a, where some other speculations concerning precognition are mentioned).

In this outrageous theory a signal is supposed to go right round the spherical universe, being roughly focussed back to its original position. (See also Appendix D.)

A somewhat theological implication of telepathic precognition was argued by Good (1964; 1965; 1969b). When I first wrote on this topic, I thought that precognitive telepathy was not much less probable than straight telepathy because of S.G. Soal's results. Now that it appears that Soal's results were fraudulent, I think precognition is much less probable. Nevertheless I give here a brief summary of the argument.

Among the closest twelve stars, there is at least one other planetary system according to van de Kamp (1963). Therefore there are probably billions of planetary systems in our own galaxy of stars. Life is highly adaptable and may well evolve in most places where the conditions are right (see, for example, Wooldridge, 1966; Sneath, 1970). So there are probably millions of places where life has evolved. It tends to evolve to greater complexity and intelligence both in individuals and in societies. (I call this the Fourth Law of Thermodynamics. The Second Law applies to closed systems, but living organisms, both individually and in groups, are open systems and of course they acquire negative entropy by eating and by receiving other forms of energy and information from their environment.) On many planets life will be ahead of us in development, and, where it is, it will usually be hundreds of millions of years ahead, for humans have only recently evolved. Such life will often be ultra-intelligent, or it might have been supplanted by ultra-intelligent machines or biomachines or disembodied fields of some kind. Such entities will have had plenty of time to take over the galaxy but the society of ultra-intelligent entities might have a serious communication problem because the diameter of the galaxy is some 80,000 light years. The entities will have perfected telepathy if telepathy is possible at all, and, if telepathic signals can be propagated with infinite speed, the whole society of ultra-intelligent entities can be in continual instantaneous communication and so the individuals can act as the cells of a vast brain, or integrated immortal consciousness. (Stanley Kubrick told me he liked this idea, and he immortalized the expression in a Playboy interview but unfortunately no acknowledgement was given. A similar idea occurs in Stapledon, 1937.) I call this integrated immortal consciousness Godd. [My present view is that Godd is unlikely to exist because precognition is improbable and therefore also so are tachyons. But I continue with the argument. Note incidentally that in Newtonian physics gravitation must travel with infinite speed otherwise the principle of conservation of energy would be contradicted: see, for example, Good (1975).] Strictly speaking Godd is immortal only if the universe is of infinite age which I believe is the case on aesthetic grounds.

There is more than one cosmological theory that assumes an infinite age for the universe. There is the Steady State theory of Bondi and Gold (1948) and the less respectable "Chinese Universes" theory of Good (1972, 1973). [I now call this, or a modification of it, the Black Hole Hierarchical Universes Theory or BHHU theory: see Theoria to Theory 10, 1976, 191-201, which discusses the matter in much more detail. Anyone who reads this reference will see that it entails modifications for the next paragraph of the present text. Roger Penrose independently proposed a similar

theory in his Adams Prize essay of 1966. He informed me of this in 1976 and said "I don't think I really believe it though!"] The Bondi-Gold theory has substantial evidence against it, whereas the BHHU theory is unfortunately not worked out in sufficient detail to be readily refutable if wrong. It might be impossible to obtain convincing observational evidence for distinguishing this theory from the standard big-bang theory. But in my opinion the BHHU theory is less ad hoc than the standard theory. To assume that our universe came from another universe is like assuming that people came from people. To assume that it suddenly came from nothing, on the other hand, is like assuming that fruit flies are spontaneously generated by bananas. The BHHU is therefore at least as reasonable a priori as the usual big-bang theory, and not more metaphysical.

The BHHU theory assumes, as did Bondi and Gold, that matter is gradually created out of nothing. Consequently any galaxy ultimately becomes so dense that it collapses under its own gravitational forces into a black hole, and at the centre of this hole the matter becomes of virtually infinite density. It then acts (according to the 1972 form of the theory) as the "aether" for a sub-universe within this black hole in which the new matter has energy of the opposite sign. (The notion of an aether of infinite negative energy occurred in the theory of the electron due to Dirac, 1947.) The process is repeated indefinitely, so there are an infinite number of positive and negative black holes forming a dendroidal hierarchy. Black holes are so weird that it would be naive to be confident that even the law of conservation of energy is satisfied when a black hole is formed. Can energy be conserved if space and time get interchanged? It seems to me to be as reasonable to assume that a new universe is created when a black hole is formed, and this is the basis of the BHHU theory.

Stapledon (1937/1953, p.401) speculates that there might be an infinite number of universes connected by something like telepathy and which he describes as "the whole dizzying hierarchy of creations," but his speculation makes no reference to black holes, and his use of the word "hierarchy" is not the same as mine. One could, however, interpret the BHHU theory as an explanation of how the "star maker" created (? creates) (? is) Stapledon's "ultimate cosmos."

Assuming the BHHU theory, the Godd associated with a galaxy would presumably avoid being sucked into a black hole where it/he/she would be annihilated. Although each galaxy has a finite life-time its Godd might be able to abandon the sinking ship and thus live forever within the universe, itself a black hole, in which he was born. If Godd did not exist Good would have invented him.

In the BHHU theory there are a countable infinity of subuniverses, and so a countable infinity of Godds.

So much for Godd, but let's go higher. For this purpose I shall again invoke quantum mechanics (see Appendix B). The usual opinion of physicists is that it is meaningless to talk about the state vector of the whole universe. For in orthodox quantum mechanics the only use of a state vector is for an observer of a system to make predictions, whereas, by definition there is no observer outside the universe. But

is it by definition? What if we assume nonpantheistically that this observer is God? Then the universe would develop deterministically except when God makes an observation upon it? (Good, 1971c.)

But suppose Wigner is right. He draws the line at the solipsism to which the total acceptance of quantum theory appears to lead, while tending to accept the theory for systems consisting of inanimate matter alone. If we believe this, we seem to be forced to believe that other conscious beings have a commonsense reality whereas inanimate matter has the more nebulous existence of quantum mechanics. Matter is aether-eal and mind is the solid rock. As stated in my editorial comment on Wigner (1962), we are here close to the "communal solipsism" of George Berkeley (1710). It is but a short step to the assumption that all minds are part of a single system that can be regarded as the Observer of the physical world. So once again we are led to the notion that the state vector of the entire physical world represents the total knowledge that God has of it. This speculation does not depend on the Branching Universe theory though it can be combined with it. The result of this combination would lead to a polytheism that would make most other forms of polytheism seem unitarian in comparison. In fact the number of gods would be countably infinite.* Unity could be reinstated only by assuming that cross-communication between branches was possible after all, leading to a Holy Aleph₀-ery.

Appendix A. Jung and Astrology

Edgar Wind, who had been Professor of the History of Art in Oxford, wrote me a letter on 12 January 1970 in which he reported on Jung's views on astrology. I quote

The conversation with Jung (which took place in the middle thirties in London) was confined to one subject - astrology. He explained that he had calculated his own horoscope and, by doing so, had learned a great deal about himself; and that he often recommended it to his patients, who likewise learned a great deal about themselves by that method. I then asked him whether he meant that astrology (as the official practitioners assert) is a science that enables you to predict future events, or merely that a horoscope can be used as a schematic substratum - just as coffee grounds or a pack of cards is used by prophetic gypsies, or a crystal by a crystal-gazer - to arouse the imagination and project into the schema certain images that unconsciously occupy your mind. He burst out laughing and said of course he meant the second, but that if he told that to his patients it would not work. I replied that, in view of the fact that I was not his patient, he should perhaps not use with me the same

* This should not be confused with the countable infinity of Godds mentioned earlier, nor with that of Zeus's discussed by Good (1969a). When a set is countably infinite its cardinality is commonly denoted by Aleph₀.

mystifying language that he might find appropriate in the consulting room. But he did not agree with that at all. What was good for his patients and for himself was good for everybody, and if I declined to calculate my own horoscope, this merely showed that I had a resistance to learning to know myself a little better.*

To say, as you suggest, that he told his patients some fairy tales in which he did not believe himself is, I think, too simple and much too rational. He believed that his schemata (or whatever you want to call the hocus-pocus) were effective, and that was all that interested him about them. His talk and his writings are not critical, in the sense in which you and I would expect a scientist to be critical. They are the effusions of a medicine-man, who has found by experiment, on himself and on others, that the calculation of a horoscope can have a cathartic effect. Therefore he recommends horoscopes, and that is all.

Incidentally, I must say in his favour that he was the only psychoanalyst of any school, whom I ever found to have a sense of humour.

It seems appropriate that I should state my own opinion about astrology. I think ordinary astrology, in spite of its romance, is extremely unlikely to be valid; even the astrologers Dean & Mather (1977), in a remarkable survey, do not claim scientific evidence for it. I can believe that the season of birth can have some effect because the first few months of life might often be the most important in the forming of the personality. When I mentioned this to a student named Deska Adams, she said that any investigation of this point should make allowance also for the geographical latitude of the birth. It is just possible that the hour of birth has a slight effect because of the known accuracy of biological clocks. (How accurately can you tell the time in the morning by the difficulty in opening your eyes?) Finally the position of Jupiter might have some effect on sunspots and so on magnetic storms, and these in their turn might have small personality effects. But, if I am right, the appropriate way to investigate a kind of astrology is to classify births by month, latitude, time of the day to the nearest hour, and magnetic storm conditions; after which the further effect of planets within zodiacs should be zero. In statistics this kind of argument is sometimes called "partialling out."

There is one possible loophole. Perhaps Godd, or God, in order to strengthen Jung's strategy, or to alleviate the boredom of normality, or just for fun, arranges matters to give astrology additional validity! Once one adopts such hypotheses, much of statistical analysis in

* Is this consistent with the comment that it "would not work" for his patients? Perhaps Jung meant (or even said) that it would work less well.

science is undermined. Scientists need to believe with Einstein, as a matter of faith, that God is not malicious, but Godd might be. ("Raffiniert ist der Herrgott, aber boshaft ist er night.") To parody a popular song, good Gods don't but Godd does.

There has been an appreciable amount of statistical analysis of astrology by Gauquelin (1973). His writings debunk ordinary astrology, but his "cosmic influences" are astrological enough for me. He obtained apparently highly significant tail-area probabilities correlating certain professions to the positions of planets in twelve sectors of the sky at the time of birth. Anyone who has time to analyze this data should appreciate that such a tail-area probability P is not strikingly significant unless PN is much smaller than 1, where N is the triple product of the number of professions entertained, the number of sectors and the number of relevant planets. (Actually he allows for pairs of sectors, but I shall come back to that.) It would also be necessary to check which of the candidates had more than one profession, and whether Gauquelin had made any marginal allocation of professions to support his case. Finally it should be held in mind that the reciprocal of a tail-area probability is usually appreciably greater than a "Bayes factor," the factor by which the initial odds of a hypothesis are multiplied in the light of the evidence. For example, a tail-area probability of $1/3000$ is often worth a Bayes factor of only about 100 [and the factor is roughly inversely proportional to the square root of the sample size; see, for example, Jeffreys, 1938/61; Good & Crook, 1974. Tail-area probabilities are often interpreted incorrectly; for example, a tail-area probability of $1/100$ is often incorrectly described as 100 to 1 against or even as 100 to 1 on!].

Since this was written there have been new investigations of Gauquelin's ideas. See, for example, "Four-part report on [the] claimed 'Mars effect'," in The Skeptical Enquirer 4, No.2 (Winter 1979/80), 19-63, and also the "Review Symposium on Astrology" in the Zetetic Scholar, Nos. 3 and 4 (1979), 71-121. Gauquelin had found that 452 out of 2088 European sports champions were born when Mars was in either sector 1 or sector 4 of his twelve sectors of the sky. The expected number by chance was only 358.5, so there was a "bulge" of 93.5 which is 5.426 standard deviations. This corresponds to a tail-area probability of 3×10^{-8} . This must be multiplied by say 100 to allow for the number of possible professions, and another factor (say 5, necessarily subjective) to allow for the ad hoc exclusion of athletes who are not the very best. I would have multiplied by a further factor of 66 to allow for the selection of the pair of sectors, but this same pair are the "key sectors" for other professions and planets, so I'll ignore this choice. Then we must allow a factor of about 7 for the selection of the planet Mars. Finally, the results for a later sample of American athletes were unconvincing, but Gauquelin has some responses to this objection. (See The Skeptical Inquirer.) He would not have needed these responses if the American sample had supported his case, so it is perhaps generous to regard the American sample as only a factor of 3 against him. The net result is to bring the effective P -value up to about $1/3000$. Since my prior probability that such effects are causal is much smaller than this, such a tail-area probability does not make me believe in Gauquelin's "cosmic influences," but it does shake me a little. There might be a better test of Gauquelin's

work than has so far been reported, for in the Zetetic Scholar symposium, p.96, Arthur Mather says

A simple test would be to take Gauquelin's own data and alter the years randomly. If the characteristic distribution persisted, then all the results would be spurious. Both those who are for and against astrology (in the broadest sense) as a serious field for study recognize the importance of Gauquelin's work. It is probably not putting it too strongly to say that everything hangs on it.

Mather does not say what he means by "everything"; maybe he means just Gauquelin's results, but perhaps at least the whole of neo-astrology hangs on it for the time being, or even the whole of parascience!

An interesting historical question is whether Hitler believed in astrology. The question does not seem to be clearly settled (Howe, 1972). It might be relevant that Hitler's invasion of Russia started on the anniversary of the French capitulation, but this might have been arranged for the benefit of Nazi morale.

Appendix B. Some elementary features of quantum mechanics

There is no question here of explaining even the elementary parts of quantum mechanics. A good exposition can be found in Gillespie (1970), who bases the theory on six postulates.* Here we shall be concerned with only about one and a half of these six postulates.

Let S be a physical system and let O be an observer. The physical system S is isolated from the rest of the world except when O makes an observation upon it. According to quantum mechanics, O can make probabilistic predictions about his next observation on S by the following technique. He assumes that the state of the system is represented by a point or vector ψ in a certain abstract space. I shall soon give some indication of how O identifies ψ . So long as O is not making an observation, ψ moves about continuously in the abstract space in accordance with a deterministic differential equation known as Schrödinger's equation. But when O makes an observation of a specific kind, such as a measurement of energy, the possible outcomes ("eigenvalues") of that measurement can be inferred by O by means of a certain algorithm which also tells him what the probabilities are of the various possible outcomes (their so-called probability distribution). Moreover, when the observation is made, the vector ψ suddenly moves discontinuously (by "projection") to one of a certain set of vectors called eigenvectors.

* I found only one important obscurity in this book. In the sixth postulate Gillespie states that a "well-behaved function of position and momentum, $f(x,p)$, is represented in quantum mechanics by the operator $(f(X,P))$." He omits to mention that the function must be symmetrized, for example, XP must be replaced by $(XP + PX)/2$. This is necessary because the product of two Hermitian operators is not Hermitian in general. The reader of the present discussion will not need to understand this point.

In many circumstances there is a one-one relationship between the possible outcomes of the measurements and the possible eigenvectors, so that the observer will know what the eigenvector has become after he has made the measurement. He can then use Schrödinger's equation to predict the future states of the system.

I have here omitted (i) a description of the abstract space in which ψ moves, (ii) the specification of Schrödinger's equation, (iii) the rules for determining the probability distribution of the outcomes of a measurement, (iv) the nature of the discontinuous change in ψ (known as the "collapse of the state vector or wave packet"). Although these details are extremely interesting, they are not strictly required in the present discussion.

All this is stranger than science fiction but it soon gets stranger. For now suppose that there is another observer O_2 who regards S plus O , assumed to be isolated, as the physical system of interest to him. From his point of view this more complicated system again has a state that is represented by a vector ψ , and this vector again moves continuously even when O makes an observation on S . If the observer O_2 believes that the state vector ψ is a genuine description of reality, then he is forced to the view that when O made his observation he (O) split into more than one person and became one person again only when O_2 made his own observation. This point is basically the same as Schrödinger's "cat paradox" (see, for example, DeWitt, 1970).

At least three resolutions have been offered for this paradox.

(i) The most popular resolution among hard-boiled physicists is to give up the idea that the state vector is a description of reality and to claim that it merely provides a basis for making predictions, justified by the great success of the predictions. (For example, the whole of classical chemistry can be explained in principle by the theory.)

(ii) Wigner's resolution is the proposal that quantum mechanics does not apply rigorously to systems containing conscious beings (Wigner, 1962). Then the observer O_2 cannot legitimately apply quantum mechanics to the system $S + O$ and the paradox is avoided. For further discussion of Wigner's theory, see Zweifel (1974), and references therein.

(iii) The Branching Universe theory, described very briefly in Appendix C.

Appendix C. The Branching Universe Theory

This appendix contains a very brief account of the history and meaning of the Branching Universe Theory, less felicitously known also as the Many Worlds Theory. Niels Bohr once said of a physical theory that it was crazy but not crazy enough to be true. The Branching Universe Theory cannot be criticized on these grounds. As DeWitt (1970) says "Although this proposal leads to a bizarre world view, it may be the most satisfying answer yet advanced." It is bizarre enough to be true.

This account is based largely on Good (1962, p.154) which, however, did not give the technical details of the theory. It is not possible to explain the theory properly without mathematics, but perhaps the present account will give some of its flavour. An official technical account of the theory is given in the book edited by DeWitt and Graham (1973).

In a nutshell the theory assumes that the universe is continually branching out into myriads of distinct universes having no communication with each other. Thus time is assumed to have the topology of a tree rather than a line. Prima facie the theory would have no observable consequences. You are unaware of the branching because at each branch-point you split up into many identical twins with whom you can no longer communicate. In the official Branching Universe theory it remains true that "the moving finger writes and having writ moves on," but there is a finger in every branch. The hand of God has the topology of a tree.

In a quarter-baked form the theory occurred first in science fiction. Isaac Asimov has kindly supplied the following references: Leinster (1934), de Camp (1940), and Asimov (1955). Yet another reference is Stapledon (1937, p.400 of the 1953 edn.). The idea was used also by the famous Argentinian writer Borges (1956/62).

In science fiction a discovery is made for crossing over from one branch to another, leading to exciting parascientific consequences. This would require that time branches backwards as well as forwards, and it would have to contain closed loops. This seems somewhat less probable than the straight branching theory, but for the moment we are discussing only possibilities.

In a large philosophical conference in Oxford in 1954 there was a session on whether it was logically impossible for an effect to precede its cause. If true this would imply that precognition was a logical impossibility. From the floor I invoked the Branching Universe theory, while mentioning the value of science fiction for the speculative philosopher. I said the theory is capable of explaining where the probabilities of quantum mechanics came from, since the probability of an event should be proportional to the number of branches along which the event occurred. Further, that the theory would remove the logical paradoxes of time travel. For if we made a change in our past, we would arrive at a different branch in the present. From this it follows that it is not logically necessary for causes to precede their effects and therefore that precognition is logically possible. This was perhaps the first "serious" use of the theory.

Three years later Everett (1957), with the support of John Wheeler, put forward a well-baked form of the Branching Universe theory as a consistent metaphysic for quantum mechanics.* In this theory it is not assumed that the state vector "collapses" discontinuously (see Appendix B), but rather that it develops continuously all the time, in accordance with a Schrödinger equation. The state vector now describes not just

*Wheeler, in Frontiers of Time (Center for Theoretical Physics, Austin, Texas, 1978, p.6) says of Everett's theory "We once subscribed to it. In retrospect, however, it looks like the wrong track."

one branch of the universe but the entire tree, and the apparent discontinuities arise only when we ignore our splitting into billions of homozygous twins at every instant. In this theory it is not necessary to think of a physical system and an observer as separate, and the state vector of the system plus the observer is supposed to be interpretable by the same observer.

There is an apparent difficulty, not mentioned in the literature as far as I know, that a deterministic system which can predict its own future can change that future, a point emphasized by MacKay (1960) though not in the present context as far as I know. MacKay's aim was to argue for the possibility of free will in a deterministic world. (Compare Edwards, 1754.) For some of my own views on free will see Good (1971 a,b). The difficulty arises in a new form in the branching universe, which be it recalled is deterministic. If you have knowledge of the fraction of the branches for which various events would occur, then this knowledge would itself be one of the causative elements affecting those fractions. It is as if one were trying to solve a set of equations whose solution was taken as one of the independent variables.

Let us make this point more explicit. Suppose that the present state of the world (at time 0), apart from your mind, is denoted by x_0 , and the present state of your mind is denoted by y_0 . The future state of the external world at time t , is denoted by x_t . Suppose that we are aware of the true deterministic laws of nature and make a prediction $x_{t,1}$ of x_t by means of equations

$$x_{t,1} = f(x_0, y_0).$$

After solving these equations, the knowledge of the solution can be incorporated in your knowledge which becomes $y_1 = y_1(y_0, x_{t,1})$, meaning that the new state of knowledge is a function of the previous state of knowledge and of the first prediction of the future. We now make a revised prediction

$$x_{t,2} = f(x_0, x_{t,1}).$$

(I have ignored here the fact that x_0 might have changed slightly because the first calculation must have taken a non-zero length of time.) We can go on revising our prediction until time catches up with us. When this happens we have failed to predict the exact future after all. Thus, by allowing for the finite speed of calculation, no paradox is reached. (Compare Good, 1971a.) This argument depends only on determinism and does not depend specifically on the Branching Universe theory.

The Branching Universe theory, though in a sense deterministic, goes some way to explaining the origin of probability in quantum mechanics. For in the modified form of the theory proposed by Graham (1973), the fraction of branches satisfying some condition is equal to the usual quantum-mechanical probability of that condition as I had suggested in my discussion at the 1954 Oxford conference, mentioned earlier. Then the branch that you go down seems to be a probabilistic matter with the appropriate probability. One is tempted to conjecture that a conscious decision involves the changing of the probabilities implied by quantum mechanics, as if you were able to send more of your

replicas down favourable branches than would be implied by the Branching Universe theory. This comes to much the same as saying that quantum mechanics might not be strictly applicable to conscious beings, though not for the reason proposed by Wigner (1962).

The Branching Universe theory as a cure for mortality. In a science fiction story the following method of travelling was invented. The traveller T would be somehow scanned in three dimensions, the information would be transmitted electromagnetically to the destination and would then be used to reassemble an exact replica T' of the traveller, complete with memories. This is like the "beaming down" in Star Trek except that there the traveller is not duplicated. After killing the original, in accordance with a contract previously signed by him, the replica would feel that he had successfully and almost instantaneously completed the journey.

Now, if the Branching Universe theory is true, when we are in a dangerous situation, where there is some physical probability but not a certainty of death, there would be survival along some fraction of the branches. The survivors would be analogous to the replica T' in the science fiction story, and they would be even better off than T'. For, since they would not believe the Branching Universe theory, they would not need to feel pity for their unlucky twins, whereas T' would have a bad conscience since he would remember having virtually committed suicide when, as T, he signed the contract. If this argument is correct most of us would live long enough along some branches so that we would be able to benefit from future giant advances in medicine. Thus some of us would become ultra-intelligent and would become a part of Godd.

For example, suppose that you were in front of a firing squad of ten soldiers. Then, with a non-zero probability, all the soldiers would suddenly die of cardiac arrest, thus enabling you to escape. This event would occur along some of the branches in which branches the event would be regarded as a miracle.

The argument can be taken further. Some of us would find ourselves having extraordinarily long sequences of narrow escapes from death while our friends and colleagues were succumbing one by one. Each person would believe that somehow he was the chosen one, not realizing that everybody else was equally lucky in other branches of the Universal Tree. But along other branches you would suffer incredible hardships.

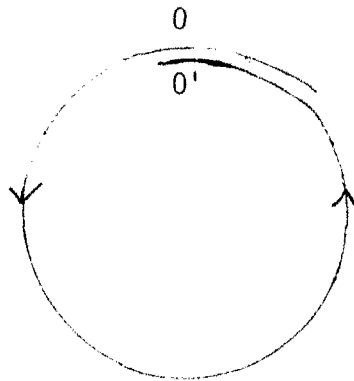
Appendix D. An Elaboration of the Whispering Gallery Hypothesis for Precognition*

The whispering gallery hypothesis is that we might be able to send telepathic signals to ourselves all the way round the spherical universe, focussed back into our vicinity as in a whispering gallery (Good, 1962a). If the signal travels a little faster than light, according to one observer, then it goes backwards in time relative to some other observers moving with respect to the first one; in accordance with the Special

* This appendix was previously distributed in mimeographed form in para-Science (Dec. 1971).

Theory of Relativity. It has been objected that it is not clear "how relativistic time-dilations or contractions can affect the before-after series for one observer..." (Chari, 1971). In order to meet this objection it might be held in mind that, owing to the expansion of the universe, after one conceptual circuit of the universe we find ourselves travelling away from our original position with the speed of light, at least to a good approximation. (The meaning is clarified in the remainder of the paragraph.) Thus each of us can be considered as at least two observers in very rapid relative motion. In accordance with the concept of "winding space," these "copies" of ourselves do not have to coincide precisely but could be relatively displaced by atomic dimensions, in a direction perpendicular to ordinary space (Good, 1962 (ii)). With or without this concept, we each might be replicated or almost replicated. Eddington (1940) thought of the replication as occurring at the antipodal point in the universe, but here we are thinking of the replication of an object 0 as occurring at the same place as 0. Fig. 1 clarifies the idea.

Fig. 1. Telepathic Signal Travels Round Universe



0' is at the same place as 0, but can also be regarded as displaced from 0 by a distance equal to the circumference of the universe. Along the latter direction, 0' is receding from 0 with the speed of light because of the expansion of the universe.

[I have not thought this idea out well enough to be sure that it makes any sense.]

If an observer is receding from us with velocity u and we send a signal to him with velocity v , where v exceeds u , then the special relativistic formula for the speed of the signal relative to the receding observer is

$$\frac{v - u}{1 - vu/c^2}$$

where c is the speed of light. If u and v are both equal to c , this relative velocity becomes indeterminate. We do not know how close u and v might be to c . Hence the return signal might be delayed by an

indeterminate time interval which could be positive or negative. If it is negative we would interpret the experience as precognitive.

A possible objection to this argument is that it uses the Special Theory when the General Theory of Relativity would be more appropriate. I would welcome criticisms from those who are at home in the General Theory.

Appendix E. Why hasn't better ESP evolved?

The question was raised, in the main text, of why we have not evolved much better powers of ESP. Targ & Puthoff (1977) suggest that our powers are latent but are suppressed by the society we live in which puts so much emphasis on analytical thinking. This, they suggest, causes the left hemisphere of the brain to become dominant, and this suppresses the intuitive ESP powers of the right hemisphere.

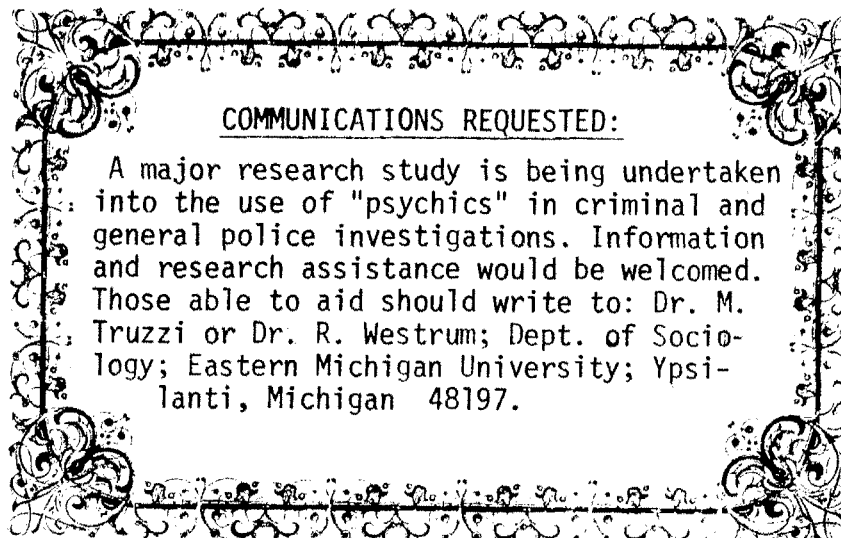
It would be interesting to do experiments using people with split brains. Such a person has a severed corpus callosum. About a dozen such people are known. They would be asked to draw pictures with their left hands because these are controlled by the right hemisphere. It should be possible to inform their right hemispheres what to think about.

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THE LOCH NESS MONSTER: A Guide To The Literature



HENRY H. BAUER

The generally unsatisfactory nature of the literature dealing with anomalies has been discussed by Westrum and Truzzi (Zetetic Scholar, 1, #2 (1978) 69-78). Their remarks certainly apply to the voluminous material concerning the Loch Ness "Monster". My aim here is to distinguish the reliable from the unreliable, and to indicate where the best evidence can be found.

Inevitably, that task involves the making of judgments, and the reader is entitled to know what my preconceptions are. The chief one is a firm belief that Loch Ness indeed harbors a population of animals related to no known extant species. That belief became firm in the early 1970s, following some 10 years of less-than-systematic interest in the subject and periodic bouts of gathering literature. I have come to know Tim Dinsdale, have seen his film on many occasions, and heard him talk to many audiences on tours in Kentucky in 1975 and in Virginia in 1979. I visited Loch Ness in 1958, 1973, 1975, and 1980, and have never seen any of the animals.

The literature about Loch Ness deals mainly with the following: modern eyewitness accounts; historical references; photographic evidence; sonar evidence; speculative identifications; reports from other lakes; accounts of sea-serpents. Potentially the most reliable evidence is photographic, and I will give special emphasis to the reliability of various photographs and to indicating where the best photographs can be found. For the rest, I shall state only whether the books are generally reliable or not. As far as identification is concerned, none of the discussions is particularly convincing---a delightful aspect of this business is that, though these animals exist, there are excellent reasons why they should not, since one can readily argue why they cannot be fish, eels, amphibians, invertebrates, reptiles, or mammals.

THE PHOTOGRAPHIC EVIDENCE (chronological)

Here are listed the photographs most commonly found in the literature, with references to the books in which they are published.

- I. Gray, 1933: A massive shape with much thinner "neck" or "tail", poorly contrasted against the water except where the object's shadow lies on the water; unanimously regarded as genuine, i.e., taken at Loch Ness, negative unretouched. Published in 1, 2, 6, 11-13, 15; published upside down in 7 and 20; a purported sketch (4) actually is only of the shadow of the object.
- II. Irvine, 1933: Still from a moving film that has been lost; shows a dark shape on or in the water, little or no sign of motion (1, 11, 12i); sketched by Burton (4).
- III. Surgeon, 1934: The most famous, apparently a silhouette of a neck tapering from considerable width at water-line to comparatively thin where it joins a small head which has suggestions of horns or ears; various patterns of ripples in the water. Almost universally accepted as showing a Nessie (1-4, 5ii, 6-9, 11, 12, 15, 16, 19, 20). Described by Mackal (13) as a bird.
- IIIA. Second Surgeon's photograph: A print discovered in the 1950s (2), the negative having been lost; magnification and focus greatly inferior to III (which was blown up from a negative), but of unquestioned provenance. Silhouette has changed angle with water compared to III (2, 11, 12). Again, said by Mackal (13) to be a bird.
- IV. Mountain, 1934: Five photographs, four showing wakes that could easily be from boats, one showing an indistinct hump with apparent spray of water (4, 11-13, 17, 20). A moving film taken by Captain Fraser subsequent to the main part of the Mountain expedition has been lost; a still claimed to be from this film shows glassy water surface, a small "head" protruding and further back a squarish-triangular "fin"; sketched by Costello (11).
- V. Adams/Lee, 1934: Details of photographer unknown, must be regarded as doubtful. Silhouette rounded at top, sharply focussed concave at left, fuzzy convex at right, considerable water disturbance at right. Very

grainy, indicating very large magnification from negative (11, 12i, 13); sketched by Burton (4).

- VI. Irvine, 1936: Stills from another lost film; some doubts have been expressed, suggestions of a mechanical model having been used. First sequence, water disturbance from left to right, one or two dark shapes appearing above water (11, 12i). Second sequence, from right to left, more water thrown up at front, less sign of an object (2, 11). Sketched by Burton (4).
- VII. Taylor, 1938: Still from movie, large and small black lumps, very indistinct in view of magnification (4, 12i). Burton, who has refused others access to the film, says it looks lifelike but is an inanimate object (4).
- VIII. Stuart, 1951: Three large, angular humps; no signs of water movement. Universally accepted as genuine (2, 4, 7, 8, 10-13, 15, 17, 20).
- IX. MacNab, 1955: Long black hump, shorter one behind, shadow of "neck" in front, with Urquhart Castle on the right; water in motion. Widely accepted as genuine (2, 3, 5ii, 7-9, 11, 12, 15, 18). Mackal (13) regards it as doubtful owing to discrepancies between previously published versions and negative supplied to him by MacNab.
- X. Dinsdale, 1960: Widely shown 16mm movie of hump, throwing up heavy wake as it moves away and to the right; wake narrows as hump submerges, then swings around and moves right to left. Universally acknowledged as genuine; Joint Air Reconnaissance Intelligence Center (Royal Air Force) estimated hump as 3 feet high above water, 12 to 16 feet long above water, "probably animate", moving at about 10 mph. Stills published by Dinsdale (3) and others (11, 12, 15). Burton's suggestion that the object is a boat is fatuous.
- XI. O'Connor, 1960: Flashlighted large hump and tubular neck; controversial. Dinsdale (3i) is inclined to accept it, Mackal (13) rejects it. Published also by Costello (II) and by Rabinovich (20), but in the latter only the hump is shown.
- XII. Cockrell, 1960: A low hump, a small black object a little distance away, broken water; not firmly accepted (3, 4, 9, 12); Mackal (13) says it is a log.

- XIII. Lowrie, 1960: A large, long wake on smooth water; taken from yacht; observers on shore also saw wake near yacht, unquestionably resulting from large object just below surface (3ii, 3iii, 5, 9, 12, 13).
- XIV. Searle (14), 1972--: Daily newspapers have published between one and two dozen of Searle's photos showing humps, neck, and head in various configurations. Some of those pictures are unquestionably faked, throwing doubt on all of them; his first (10), and perhaps one or two others, could be genuine---but hardly those published by Akins (17) and Cornell (19).
- XV. Rines/Edgerton "flipper", 1972: Underwater; diamond-shaped fin attached to large object extending beyond film frame (9, 12, 13, 15, 16, 19). Less widely published a second picture taken about a minute later, showing altered position of flipper (13, 16).
- XVI. Rines "body-neck", 1975: Underwater; front or side of body, two appendages plus long neck whose middle is not illuminated but seems to terminate in illuminated rounded "head" with suggestion of mouth-line (13, 15, 16); published in inverted form (left and right transposed) by Rabinovich (20).
- XVII. Rines "gargoyle", 1975: Underwater; bizarre "head"---open mouth, teeth (?), bony ridges, 2 antennae or horns (13, 15, 16); published upside down and inverted by Rabinovich (20).

BOOKS (chronological)

1. Rupert T. Gould, The Loch Ness Monster and Others, Geoffrey Bles 1934; reprinted by University Books, 1969.

Classic and recommended. Some 50 eyewitness reports, many sketches, photos I-III. Gould's perspicacity is evidenced by his conclusions, borne out by decades of further investigation: Nessie is up to 45 feet long, diameter 4 to 5 feet, has at least one pair of paddles, a ridged back, dark and warty skin; and is a land-locked sea-serpent. (But Gould thought in terms of a single creature, now the notion is of a breeding population isolated from the sea thousands of years ago.)

2. Constance Whyte, More Than a Legend, Hamish Hamilton, 1957.
Also classic, recommended, reliable. Many eyewitness reports personally gathered by author, who had lived for 20 years in Inverness. Relevant folklore and historical accounts; reports from other lochs, other countries, and of sea-serpents. Photos I, III, IIIA, VI, VIII, IX.
- 3i. Tim Dinsdale, Loch Ness Monster, Routledge and Kegan Paul, 1961.
Chiefly personal story of Dinsdale's awakening interest, preparations for trip, success in filming Nessie, and attempts to interest professional biologists. Survey of folklore, history, other lochs and countries. Photos III, IX-XII.
- ii. ----- second ed., 1972.
Completely revised; omits accounts from other places (which are treated in more detail elsewhere (5)) and adds reports of activities at Loch Ness since 1960. Omits photo XI, includes photo XIII.
- iii. ----- third ed., 1976.
Appendix brings story of investigations at Loch Ness up to date.
4. Maurice Burton, The Elusive Monster, Rupert Hart-Davis, 1961.
A sad and infuriating book. Burton had been the only well-known zoologist to remain open-minded about the evidence from Loch Ness, and entertained and publicly discussed such possibilities as giant eels and plesiosaurs. Here, he denigrates the observations and writings of others, often by ludicrously specious argumentation and by misquotation, innuendo, and the like. He ascribes almost all the sightings and photos to vegetable mats, escaping gases, birds, otters, and so forth; but does not discount the handful of sightings reported on land, suggesting the existence of a 20-ft. long-necked otter-like creature best sought on land. Photos III, IV, VII, VIII, XII; sketches of photos II, V, VI; the sketch of photo I is thoroughly misleading.

- 5i. Tim Dinsdale, The Leviathans, Routledge and Kegan Paul, 1966.
Water "monsters" reported around the world; events at Loch Ness between 1961 and 1965; photo XIII.
- ii. ----- Monster Hunt, Acropolis, 1972.
Revised edition of 5i; foreword by Robert H. Rines, new chapter about Loch Ness from 1966 to 1972, photos III and IX added.
6. F. W. Holiday, The Great Orm of Loch Ness, W. W. Norton, 1969.
A mixed bag. Very good about skeptics and, in particular, Burton's volte face; Holiday's own sightings, gathering of reports. But relates bizarre tale of the secretly guarded existence of two movies, one showing a Nessie in great detail, another showing a similar creature in a sea-loch; a tale repudiated by the individual who was claimed to be Holiday's source. And Holiday firmly holds that Nessies are invertebrates, related to "Tullimonstrum"---a recently discovered (in America!) fossil some 20 times as small as the Nessies. . . . Has photos I, III.
7. E. D. Baumann, The Loch Ness Monster, Franklin Watts, 1972.
Poor. Though not so inscribed, reads as though intended for young readers. No references or bibliography; inaccurate on a number of details; photos I (upside down), III, VIII (ascribed to "Stewart" instead of Stuart), IX.
8. Elizabeth Montgomery Campbell with David Solomon, The Search for Morag, Tom Stacey, 1972.
Evidence for creatures similar to Nessies residing in Loch Morar. An important and reliable book; more than 30 eyewitness reports, 20 since 1964; very good review of earlier works dealing with Loch Ness; photos III, VIII, IX.
9. Tim Dinsdale, The Story of the Loch Ness Monster, Target, 1973.
For younger readers, but can be read by all with enjoyment and profit; thoroughly reliable, good bibliography; photos III, IX, XII, XIII, XV.

10. F. W. Holiday, The Dragon and the Disc, W. W. Norton, 1973.
Far-flung speculation unconfined by facts or reason; not to be recommended. Mixes Nessies with UFOs (hence the book's title); and with the wisdom of the ancients ("still in advance of formal scientific attitudes"), with megalithic structures, ley lines (compared to the Martian canals), and so forth. Occult "explanation" of Nessie's ability to avoid clear photography, by staying out of range, or causing camera malfunction, or having the extant films kept from public view . . . photos VIII, XIV.
11. Peter Costello, In Search of Lake Monsters, Garnstone, 1974.
Good: for references, bibliography; table of 17 land sightings, table of 27 photos; JARIC report on Dinsdale film; photos I-III, IIIA, IV-VI, VIII-XI. BAD: incredibly inaccurate in innumerable details (despite the good bibliography!); laughable attempts to see similarities between different photos; idiosyncratic and bizarre assertions---e.g., the "antennae" or "horns" are actually ears (which can be 16 inches high and 8 inches long). Should not be relied upon.
- 12i. Nicholas Witchell, The Loch Ness Story, Terence Dalton, 1974.
Highly recommended as reliable, comprehensive but concise review of sightings, investigations, hoaxes; profusely illustrated with scenic and historical illustrations; photos I-X, XII, XIII, XV. Revised edition (1976) gives more about the 1975 photographs by Rines and the aborted symposium to discuss them.
- ii. ----- Penguin, 1975.
Shortened and revised version of 1974 edition; omits some historical and scenic photos, and II, V-VII; postscript describes Rines' 1975 photographs. Recommended.
- 12A. Tim Dinsdale, Project Water Horse, Routledge and Kegan Paul, 1975.
A must for aficionados; not about the animals and the evidence, but about the search and the searchers, by the single most experienced and knowledgeable monster-hunter; an incredible collection of ingenious attempts and devices, on expeditions within sight of roads and houses yet lonely and genuinely dangerous to life and limb. Good bibliography.

13. Roy P. Mackal, The Monsters of Loch Ness, Swallow, 1976.
Valuable collection of data: more than 250 sightings judged reliable, and correlations with time of year, time of day, weather, etc.; discussion of 17 still photographs and 15 movies, a number of them never published; full bibliography, index. BUT marred by self-serving tone, apparently intended to emphasize the dust-jacket's description of Mackal as "the first scientist to take seriously the problem of Loch Ness". Lengthy, inconclusive, labored discussions of what these animals might be, from umpteen different points of view---virtually a parody of "scientific" responsibility; somehow manages to make this subject dull---boring reading even for aficionados. Photos I, III-V, VIII, IX, XI-XIII, XV-XVII.
14. Frank Searle, Nessie, Coronet, 1976.
To be treated as a curiosity only; by a man who has camped at Loch Ness since 1969, has had many photographs published in newspapers, proclaims himself the only dedicated searcher, but is now regarded by knowledgeable persons as one who fakes photographs. The book has no bibliography, index, or references; has no mention of Dinsdale or his film; denigrates all and sundry. Caveat emptor: can read quite plausibly if one knows little or nothing beforehand; but the publishers have refused to keep the book in print, having been convinced that the author is unreliable. The only photos are some of Searle's own, and include one of the most unconvincing of his fakes.
15. Jeanne Bendick, The Mystery of the Loch Ness Monster, McGraw-Hill, 1976.
For youngsters. Very reliable. Photos I, III, VIII-X, XV-XVII.
16. Dennis Meredith, The Search at Loch Ness, Quadrangle, 1977.
Well written account of the controversy and confusion over Rines' 1975 photographs, and of the 1976 expedition; enjoyable reading, and also a useful source for sociologists of science interested in this classic case of "resistance by scientists to new discovery".

17. William Akins, The Loch Ness Monster, Signet, 1977.
Indications are that this was written as a potboiler, to cash in on the widespread interest in this and similar subjects. Within that genre, it is really quite good---reasonably accurate, reasonable bibliography, tables of water and land sightings, photos IV, VIII, XIV. But adds nothing to the much better earlier books.
18. Alan Landsburg, In Search of Myths and Monsters, Bantam, 1977.
More potboiling, by the infamous producer of the totally unreliable TV series "In Search Of. . ." The short piece on Loch Ness is actually not bad, neither are the sections on other fresh-water "monsters" and on sea-serpents. The yeti - Sasquatch - (swamp-ape) is also discussed. But cannot be recommended.
19. James Cornell, The Monster of Loch Ness, Scholastic Book Services, 1977.
And worse potboiling. Not so marked, but reads as though for young readers. No index, no references, badly incomplete bibliography. Asserts, without citation, some things that are simply wrong. Photos III, XIV, XV. Unreliable and to be avoided.
20. Ellen Rabinovich, The Loch Ness Monster, Franklin Watts, 1979.
For young readers; scandalously unreliable and a disgrace to the publishers, belying the attractive presentation. Photos I (upside down), III, IV, VIII, IX, XI ("neck" cropped off entirely), XVI (inverted left to right), XVII (upside down and inverted left to right). Ludicrously wrong sketch of how the loch was cut off from the ocean; photo III is described as showing a "huge body" as well as the neck; Nessiteras is spelled "Nessitara". Not to be shown to youngsters even under parental guidance.

Through lack of familiarity with the Danish language, I cannot assess the book by Palle Vibe, Gaden I Loch Ness, Rhodos, 1970; but it is interesting at the least for having the only published print of the still claimed to be from Captain Fraser's movie (IV) of 1934.

OTHER REFERENCES

There are a few shorter works, and books dealing with related creatures, that deserve mention here: because they are unusually significant, or because they are not readily available (so an outline would presumably be welcomed by those who cannot obtain them). These references are selected from a host of magazine articles and chapters or sections of books, the vast majority of which add nothing important to the data about Nessies contained in the books mentioned above.

A. Loch Ness

A. C. Oudemans, The Loch Ness Animal, E. J. Brill (Leyden), 1934; 14 pp.

A polemic against disbelievers in sea-serpents. Reiterates the thesis of his book (see below) that sea-serpents are long-tailed, long-necked pinnipeds (i.e. belonging to the seal family). Really has nothing specific about Loch Ness, simply refers to then-extant reports.

Proc. Linnean Soc. London, Pt. I, (1934) 7-12 (8 Nov.).

Discussion following showing of Mountain/Fraser movie. Significant for lack of consensus---some are sure it is a seal, others that it is an otter, others that it is definitely neither.

Underwater Search at Loch Ness, Academy of Applied Science Monograph no. 1; authored by Martin Klein, Robert H. Rines, Tim Dinsdale, Lawrence S. Foster (Academy of Applied Science, Belmont, Mass. 1972); 104 pp.

Describes sonar and underwater-photography experiments during 1970 and 1971; and the history and purposes of the Academy of Applied Science. Significant background for the later spectacular successes achieved by the Academy at Loch Ness.

Victor Perera, The Loch Ness Monster Watchers, Capra Press (Santa Barbara), 1974; 44 pp.

An essay written after a visit to Loch Ness. Nothing here for those who want facts or references, but it is a charming account of personal impressions.

Nature, 258 (1975), 11 December, 466-468.

Sir Peter Scott and Dr. Robert H. Rines propose the scientific name Nessiteras rhombopteryx, common names the Nessie or Loch Ness monster, on the basis

of photographic and sonar results and to permit full protection of the animals under the Conservation of Wild Creatures and Wild Plants Act.

Robert H. Rines, Charles W. Wyckoff, Harold E. Edgerton, and Martin Klein, Technology Review, 78 (1976) no. 5, March/April, 25-40.

A feast of photographic facts: the underwater photos of 1972 and 1975 in color; both "flipper" shots after computer enhancement, body-neck, gargoyle head, and some other tantalizingly unassigned bits.

Nigel Sitwell, Wildlife, March 1976, 102-109. In large part duplicates the information in the two preceding references; but in addition shows the original color photos of 1972 before computer enhancement.

Roger Grimshaw and Paul Lester, The Meaning of the Loch Ness Monster, Centre for Contemporary Cultural Studies, University of Birmingham, B15 2TT; 1976, 42 pp, price 40 pence.

Explores the symbolic significance of the Loch Ness monster during different cultural eras and in different settings. An interesting theme that deserves fuller and more decisive treatment.

NIS (Ness Information Service) Nessletter; ed. R. R. Hepple, Huntshildford, St. Johns Chapel, Bishop Auckland, Co. Durham, England DL13 1RQ; 6 issues p.a. beginning Jan. 1974, sub. \$8 (U.K. L2).

The only current attempt to provide continuing coverage of events at Loch Ness (with occasional items from other places). "Only" means that the newsletter put out by Searle (XIV and 14, above) cannot be recommended.

B. Chiefly about lakes in other countries

Charles E. Brown, Sea Serpents: Wisconsin Occurrences of these Weird Water Monsters, Wisconsin Folklore Society, Madison 1942; 10 pp.

Repeats local tales of water monsters, treated as fables; no references apart from a couple of newspaper reports.

F. M. Buckland, Ogopogo's Vigil; privately published 1948, reprinted 1966 by Kelowna Branch, Okanagan Historical Society; 124 pp.

History of the Okanagan region; nothing about Ogopogo the water-monster.

Dorothy Hewlett Gellatly, A Bit of Okanagan History; 1932, revised Centennial Ed. 1958, Kelowna, British Columbia.

Chapter IV (pp. 22-27) is about Ogopogo: Indian legends, and some sightings reported by settlers.

Mary Moon, Ogopogo, J. J. Douglas (Vancouver), 1977; 195 pp.

Detailed accounts of sightings, with many citations of the newspaper reports; also, a chronological summary from Indian legends up to August 1976, when 5 photographs were taken---but the one published is clearly a wave phenomenon. Another, anonymous, photograph is puzzling at best. Unfortunately no index or bibliography.

Elizabeth Skjelsvik, Folkelivsgransking, 7 (1960) 29-48 (in English).

Discusses folklore about lake and sea serpents. On one occasion, an "upturned-boat" monster-hump was found to be a mass of rotting plants, mud, pine needles, and sawdust. Skjelsvik extrapolates from this: reports of lake monsters in Norway seem to date from the 16th century, when water-driven saw-mills came into use; and were very numerous in the 19th century, when that industry expanded greatly. Burton (4) drew on this to ascribe many Loch-Ness sightings to vegetable mats, which have never been found there.

Knut Svedjeland, Storsjoodjuret, S-Forlaget (Ostersund), 1959; 62 pp. (in Swedish).

The illustrations have a jocular appearance, but the text treats in a matter-of-fact manner reports of large animals in Lake Storsjon. Generally like a "chain of barrels", 9 to 14 meters long and 1 meter in diameter; accounts cover the period 1800 to 1959, by credible witnesses, each sighting observed by no fewer than 2 individuals. (Kindly translated for me by Mrs. Ellsworth Fuhrman).

C. Sea Serpents

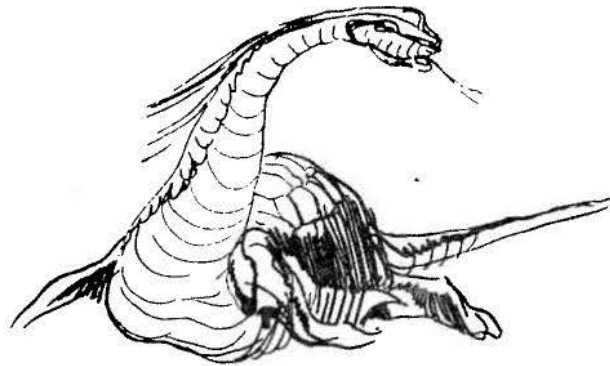
Bernard Heuvelmans, In the Wake of the Sea Serpents, Hill and Wang, 1968.

Classic and indispensable, comprehensive and detailed, fully referenced.

Rupert T. Gould, The Case for the Sea-Serpent, Philip Allan, 1930.

Another testimonial to an independent and clear-thinking man-ahead-of-his-time. Gould's work shows that at least one person was able to extract objectively accurate data from eyewitness testimony by using common sense and critical analysis and steering between gullibility and outright skepticism.

A. C. Oudemans, The Great Sea-Serpent, E. J. Brill (Leiden), Luzac & Co. (London), 1892; 592 pp. Really of historical interest only. I: ten pages, chronological list of earlier literature. II: hoaxes, etc. III: would-be sea serpents. IV: conclusions---in great detail; size, shape, skin, colors, sexual differences, senses, nutrition, etc. Also, 82 illustrations.





LIVESTOCK MUTILATIONS

A National Mystery



RICHARD H. HALL

A rancher finds his prize cow dead, its sexual organs neatly excised, along with its tongue, or an eye, or an ear. They appear to have been cut with a sharp instrument. There are no tracks or signs of a struggle around the carcass, and there is a curious lack of blood. The rancher and investigating sheriffs are baffled. Occasionally someone says that mysterious lights or helicopters have been seen in the area. This, at least, is the stereotype.

From a detailed examination of animal mutilation reports, it appears that for at least the past 7 years, someone has been maliciously carving up American farm animals for purposes unknown. Select organs are missing, and the carcass is left to rot. Many cases are explainable by predators attacking animals killed by disease or cold weather. But suggestions that this can explain the entire mystery are inadequate, and it won't wash with concerned ranchers and farmers in the seriously affected Great Plains region.

One of the most striking features of the mutilations is the alleged surgical precision in many cases. The mutilation of cattle in northwest Arkansas in 1978 was "highly skilled...very neat work by somebody who knows anatomy as well as surgical technique," said Dr. C.S. Hatfield, Bentonville veterinarian. "It was strictly done with a knife," said Sheriff's Lieutenant Alfred Carreon of a 1975 case in Bexar County, Texas.

On April 20, 1979, at Albuquerque, N.M., Senator Harrison Schmitt and United States Attorney R.E. Thompson presided over hearings attended by over 200 ranchers, law enforcement personnel, and other interested parties. Why? Because in 1978 a wave of bizarre mutilations occurred across northern New Mexico, some on Indian reservations (Federal property), and citizens wanted action.

Early in 1979 the incidents spread to the southern part of the state. A few weeks after the hearings a grant from the Law Enforcement Assistance Administration enabled the State of New Mexico to hire a recently retired FBI agent to conduct a coordinated investigation. At this writing his work is still in progress.*

As in the UFO mystery, large numbers of reports appear to be false alarms and a core of more substantial cases tends to get lost in the shuffle. Strong evidence, including some field experiments by investigating authorities, indicates that natural predators (coyotes, birds, and insects) can and do mutilate already dead cattle in ways closely resembling the alleged "mystery" cases. But sometimes this explanation is strained. For instance, when the owner knows the animal was healthy, when there is no apparent cause of death and no typical traces of predators, and when the circumstances at the site clearly suggest otherwise.

Popular writers have attempted to link the events with everything from "mystery helicopters" to UFOs, whereas owners and

* This study by Kenneth M. Rommel, Jr., is now available and will be reviewed in a future issue of ZS. -- MT

investigators--when they have expressed an opinion--have inclined toward cultist activity as an explanation. Indeed, many cases have bizarre, ritualistic elements.

In an effort to unscramble this mystery, I recently undertook a pilot study in the files of the International Fortean Organization (INFO) in College Park, Maryland. INFO perpetuates the work of Charles Fort who in the 1920's and 1930's chronicled recurring events that he felt science was ignoring. Among other things, Fort mentioned cattle mutilations in Kenya and England early in the 20th Century.

The "data" of my sample consist of newspaper reports, and due allowances must be made for the distortions that result from the pressures of daily deadlines. For the 5-year period of 1974-1978, I found 50 reasonably specific cases and two generalized series of cases involving 78 mutilated animals in 9 states. Single animals were mutilated in 39 cases, and 11 involved more than one animal. The animals were 69 cattle, 4 horses, 2 dogs, 1 bison, and 2 unspecified farm animals (probably also cattle).

The Great Plains Circle

One startling result of the study was identification of what could be called the "Great Plains Circle" (perhaps a pseudoconstruct based on an incomplete sample) within which 87% of the mutilations occurred. If one were to place a compass at Clinton, Oklahoma (just west of Oklahoma City) and draw a circle of 350-mile radius, this circle would contain 68 of the 78 mutilation sites. All that can be said is that some weird events have taken place within that circle.

Malcolm, Nebraska; September 29, 1974. A 700-pound Black Angus steer was found shot (confirmed by autopsy) and with its throat slit, but no meat was taken. A few feet away lay two dead chickens. Markings on the ground indicated that the steer had been dragged through the pasture, under a fence, then doused with a flammable liquid and set afire.** A stretch of 250 feet along a roadside ditch was scorched by the flames. Owner Larry Hudkins said, "I hate to say it, but it was set up in a sort of ritualistic manner. It's really weird."

The killing of an animal is the means by which its consecrated life is 'liberated' and thus made available to the deity....Basic to both animal and human sacrifice is the recognition of blood as the sacred life force in man and beast. Thus its great potency has been utilized through sacrifice for a number of purposes--e.g., earth fertility, purification, and expiation.... (ancient sacrifices often involved black animals).... Along with libation and the sacrificial effusion of blood, one of the commonest means of making an oblation (ritual offering) available to sacred beings is to burn it....The common place of sacrifice in most cults is an altar. However, the table type of altar is uncommon; more often it is only a pillar, a mound of earth, a stone, or a pile of stones.--Encyclopedia Britannica entry on Sacrifice, 1969

Sulphur Springs, Texas; January 1975. A pregnant Black Angus cow was found split open through the udder all the way to its front legs. The unborn calf was removed and the womb was half removed, half pitched over the calf. The cow's head had been cut off and was missing. Her jugular vein had been "cleanly cut," yet there was no blood near the carcass suggesting that it had been drained into a container. (In November 1975 in Jackson County, Missouri, another unborn calf was "removed by a sharp cutting instrument," leaving no trace of blood. The cow was found with a rope around its neck tied to a nearby tree on a secluded section of the owner's property.)

In 1979 the mutilations spread to Alberta, Canada, where Col. Lyn Lauber of the Royal Canadian Mounted Police said they were maintaining close contact with American authorities. "Ten head of cattle, mostly bulls, have been mutilated since Aug. 13. Most died after their sexual organs were removed. Police investigators are looking for a satanic or religious cult....In each case, it's clear the organs were removed by a knife or other sharp instrument," Col. Lauber said."--Toronto Star, October 2, 1979

Kiowa, Colorado; September 1975. A 700-pound Black Angus heifer was found freshly dead, its rectum removed. "The hole was almost a perfect, smooth circle where the blade had been." The heifer had been eating grass when it died; strands of grass were hanging from its mouth.

Dr. Alan Aycock, a professor at the University of Lethbridge, says the mutilations sound like a revival of 19th century English clubs for the rich and bored who indulged in witchcraft as a hobby....Aycock, who teaches a course in the occult, says organs removed from the livestock may indicate the cult wants to incorporate the animal's power. Mutilation of other parts may represent attacks on people....Cattle have traditionally been used for sacrifice, Aycock says. Often some aprts would be devoured by worshippers and others offered to their god.--Vancouver Sun, October 16, 1979

Benton, Arkansas; April 3, 1978. Two dogs were found dead with their intestines, hearts, and other internal organs removed in what the sheriff's department described as a possible ritualistic slaying.** The dogs were found inside two triangeles made of sticks. Deputy

** These and other elements of the mutilations suggest the activities of a sacrificial cult.

Walter Wilson said that unidentified people in robes had been reported seen in the area. Later that month the sheriff's department was investigating other ritualistic slayings in Saline County; "What appears to be altars made of stones ** have been found near the dead animals." In Bentonville, farther to the northwest, Lieutenant Don Rystrom said animal skulls, candles, and apparent altars of stone painted dark blue had been found, some with symbols (not described) painted on in white.

Espanola, N.M.; November 26, 1978. In two nearly identical cases, one at Hernandez and one at Chimayo, cows were found with their rectums, sex organs, and tails removed. At Hernandez, the cow was in a corral about 150 feet from the house. Its head was stuck between the fence boards, and all of the organs were gone from a "very round" 5-inch diameter hole apparently cut with a knife. There were no tracks and no blood on the cow or nearby. On the same night, the family dog disappeared. At Chimayo where the same effects were visible, the owner said, "It looked like they had gone around it with a knife." Again, there was no blood and no sign of a struggle.

These cases epitomize the broader mystery: despite severe mutilations no blood spattered around, no predator tracks, and no signs of a struggle. Some authorities are exploring the theory that the animals have been drugged. Even more startling are the indications, confirmed by veterinarians in some cases, that the blood was drained from the body before the mutilations. It is worthy of note that the owners considered the circumstances sufficiently strange to call in police investigators.

Whatever the significance might be, 75% of the mutilated animals were female. Could this suggest a fertility cult? In order of frequency the mutilated organs were sex organs (34), tongues (20), udders (19), eyes (15), ears (15), rectums (14), and tails (8), plus removal of internal organs in a number of cases. No apparent preference for left or right emerges in the excision of eyes or ears.

The tendency was strongly for multiple mutilations (83%). Only 17% had a single organ removed, and 58% had either two or three organs removed. There were few patterns suggestive of selectivity, but one was striking: one or both eyes were removed in 15 instances and in 12 of these the sex organs also were removed. (In the remaining three instances the tongue was removed.)

Mutilation Theories

In exactly the same way that multiple causes and misidentifications tend to inflate the UFO mystery, the animal mutilation mystery is inflated by a form of hysteria or a will to believe causing many false alarms. Nevertheless, the notion that nothing but predator activity is involved fails to take into account the type of evidence reported here. Non-human predators could not shoot a steer and set it aflame; rip up an animal without spattering blood around; make

"surgical" incisions; place triangles or stone altars; or wrap a rope around a cow's neck and tie it to a tree.

Conclusion No. 1: Human mutilators are involved.

Some theorists have suggested that, considering the price of beef, cattle rustlers are responsible. Rumors persist of unmarked and often modern, quiet-running helicopters (implying wealthy interests) being seen near mutilation sites. Since there is little evidence of beef being spirited away, the motive would have to be intimidation of rival ranchers, or a cover--or distraction--for some other activity. Only 4 of the 50 cases in this sample mentioned "mystery helicopters" and 3 of these were only indirectly associated with mutilations. The one exception, however, is intriguing.

On February 25, 1975, a rancher in Leming, Texas (near San Antonio), watched from about a quarter of a mile away as a "newer style helicopter" making a "screaming" or "whistling" sound landed in a grassy area near an apparently dead cow on his neighbor's ranch. Next day the neighbor found one of his cows with its tongue, udder, and sex organs removed, the udder excised with a "clean, neat cut." A sheriff's lieutenant said, "It was strictly done with a knife." The San Antonio News reported that the mutilations were the same as found on four other cows in south Texas over the previous 2 weeks.

Overall, the evidence of "mystery helicopters" associated with mutilations is weak--at least in this sample. Any connection with UFOs also is weak. "Lights in the sky" (some sounding suspiciously like stars or planets) were mentioned in only two cases.

If not predators, rival ranchers, or UFOs, what does that leave? The only theory that seems to apply to the core cases--and it remains to be proven--is some form of cultist activity. Both Satan worshipers and fertility cults have been mentioned.

In connection with the September 1974 mutilations in Nebraska, a university professor who also teaches witchcraft courses described the incidents as "ritualistic," possibly the work of a fertility cult. At Sulphur Springs, Texas, in January 1975, County Sheriff Paul Jones expressed the opinion that a Satanist or occult group was responsible. During the February 1975 Texas cases, a self-professed witch attributed the east Texas cases to Satanist groups, suggesting that the blood and sex organs were used in ceremonies. In August 1978 a university anthropologist in northwest Arkansas said the area was known as a center of witchcraft activity.

Conclusion No. 2: Sacrificial cults (possibly Satanists and witches) are responsible for some, if not all, of the clear-cut mutilations by human hand.

A female bison was found October 22, 1975, at the Colorado Springs zoo with sex organs, udder, and one ear removed. They were excised "apparently surgically," said Dr. Rodney Walker, zoo veterinarian. In the 1978 Arkansas cases Dr. Gary France, veterinarian, performed autopsies on three mutilated animals. He described the

mutilations as "precision cutting" and said it appeared that a vacuum bottle had been used to remove the blood. His colleague Dr. Hatfield, cited previously, said that in one animal the blood apparently was flushed out with a saline solution. Similar descriptions of "surgical" incisions come from at least six states as far removed as Oregon and Arkansas, so cannot be attributed to one locale or region which might have a "mad doctor" at work.

Conclusion No. 3: Persons with surgical skill and medical knowledge are involved over a wide area of the United States.

The present sample includes an average of only 16 cases per year, but the true figure is much higher. Many of the reports alluded to numerous other cases in the county or state within some recent time period. For each case that made the newspapers, it would be a conservative assumption that, on the average, five did not.

In response to a survey by the author, the Colorado Bureau of Investigation confirmed receiving 203 reports in 1975; however, most of these were attributed to predator attacks. The present sample includes only three 1975 Colorado cases.

The Texas Rangers, although unable to provide statistics, made the following interesting comment in a letter dated April 2, 1979:

"Most of these cases were reported in Texas approximately three to four years ago (1975 or 1976--editor). At that time, there were many cattle mutilations reported, some of which were attributed to suspected "Satanic Cults." Most of the cases in which "cult" rites were suspected were reported in the Panhandle area of Texas...."

The present sample includes 16 Texas cases for 1975, none from the Panhandle area. (The one 1978 Texas case, however, was in the Panhandle near Amarillo. A 4-year-old sorrel mare was found about 200 feet from the ranch house, its rectum neatly "cut out" and its teats removed. No tracks, signs of struggle, or blood was visible at the site. Deputy Art Burton said he was "totally mystified and stumped.")

Conclusion No. 4: The scope of the phenomenon--conservatively about 400 cases in 5 years--and the similarity of patterns over a wide geographical area suggest organized, purposeful activity.

Attitudes on the subject have hardened into a spectrum ranging from "predators did it" on one end, to "UFOs are responsible" on the other. The truth probably lies somewhere in between. A much larger scale study is needed, one that focuses on the truly puzzling cases rather than on numbers, and one that obtains more testimony from veterinarians, along with better documentation of each case. Meanwhile, good reason exists to suspect the existence of a widespread sacrificial cult (as opposed to fertility or other cults). If it is true that the mutilators are helicopter-equipped, this would have sinister implications. A systematic national investigation might supply the final answers.

Animal Mutilation Cases, 1974-1978, by Year and State

	TEX	ARK	NM	MO	NEB	COL	KANS	VA	ORE	TOTALS
Cattle (millions)										
1977	15.8*	2.5	1.5	6.4*	6.5*	3.0	6.4*	1.6	1.5	122.8 (U.S.)
Livestock farms (thousands)										
1974.....	53.3*	9.5	4.6	41.6*	25.9*	8.3	20.4	9.9	5.4	493.8 (U.S.)
Mutilation cases										
1974.....	1				8					9
1975.....	16			1		3				20
1976.....				2						2
1977.....				4			1		1	6
1978.....	1	10	11	6		2	2	9		41
Totals.....	18	10	11	13	8	5	3	9	1	78

* = Among top 5 states

Note: The most recent data indicates that the livestock business in these 9 states represents about 36% of the national total.

(Source: Statistical Abstract of the United States, 1979)

Animal Mutilation Cases, 1974-1978

(*multiple case) (N, X = not specified) (L=left, R=right)

Date/Loc	Animal	Ear	Eye	Tongue	Rectum	Sex	Org.	Tail	Udder	Notes
1. 5/11/74(*) Custer Co., NE	cow & calf							x	x (elit)	(e)
2. 6/7/74 Madison, NE	cow					x			x	(e)
3. 8/11/74 Lancaster Co., NE	cow	x(R)	x(R)		x	x	x		x (tests)	
4. 8/18/74 Arona, NE	cow		x (n.s.)			x	x	x	x (tests)	(d)
5. 8/21/74 Raymond, NE	cow				x	x				
6. 9/7/74 Pierce Co., NE	calf					x				(e)
7. 8/29/74 Malcolm, NE	steer									shot, throat slit(c) (d)
8. 11/7/74 Justin, TX (Ft. Worth)	cow								x	
9. 1/7/75 Breckenridge, TX (Ft. W.)	bull					x				
10. 1/7/75 Sulphur Spg., TX (Ft. W.)	cow					x			x	(e) (d)
11. 2/7/75 Pearce, TX (San Antonio)	cow			x					x	
12. 2/20/75 Copperas Cove, TX (Ft. Hood)	n.s.	x (LR)	x (LR)			x				(e)
13. 2/20/75 Hondo, TX (San Antonio)	cow			x		x	x	x	x	(b)
14. 2/23/75 Hondo, TX	cow								x	(b)
15. 2/26/75 Leming, TX (San Antonio)	cow			x		x		x	x	(b)

continued, over.

Animal mutilation cases, continued:

Date/Loc	Animal	Ear	Eye	Tongue	Rectum	Sex Org.	Tail	Udder	Notes
16. 2/27/75(*) Harris Co., TX (Houston)	cow n.s.		x(LR) x(L)	x		x			
17. 2/27/75(*) Harris Co., TX	cow horse					x			(a)
18. 2/28/75(*) Pearland, TX (Houston)	cow steer pony			x x x		x x x		x	
19. 3/17/75(*) Harris Co., TX	calf Angus		x(LR) x(LR)	x x		x x			(a) (d)
20. 7/7/75 Washington Co., CO	heifer	x(LR)x(L)			x	x			
21. 9/7/75 Kloms, CO (Denver)	heifer				x				(b)(d)
22. 10/22/75 Colo. Spgs., CO (zoo)	female bison	x(R)				x		x	(b)
23. 11/7/75 Jackson Co., MO (Kans. City)	cow							x	(b)
24. 9/7/76(*) Freeman, MO (Kans. City)	cow cow	x(L) x(L)				x x			(b)
25. 6/14/77 Conway Spgs., KS (Wichita)	calf					x	x		(b)
26. 8/17/77 LaGrande, OR	cow								parts (b) (d)
27. 9/25/77 Peculiar, MO (Kans. City)	cow	x(L)							
28. 10/4/77 Peculiar, MO	heifer	x(L)		x					
29. 10/12/77 Peculiar, MO	steer								n.s.
30. 12/13/77 Blum Spgs., MO (Kans. City)	calf	x(R)					x		(b)

Date/Loc	Animal	Ear	Eye	Tongue	Rectum	Sex Org.	Tail	Udder	Notes
45. 10/6/78(*) Dulce, NM	4 cows			x	x	x			patches of skin burned
46. 11/6/78 Cave Spgs., AR	calf		x (n.s.)	x		x			
47. 11/11/78 Española, NM	cow	?		?	x			x	(b)
48. 11/13/78 Española, NM	cow	?		?	x			x	(b)
49. 11/12/78(*) Bentonville, AR	calf calf ?								parts n.s. (a) (b)
50. 11/26/78(*) Española, NM	cow cow				x x	x x	x x		(b) (b)

Date/Loc	Animal	Ear	Eye	Tongue	Rectum	Sex Org.	Tail	Udder	Notes
31. 4/3/78(*) Benton, AR (Little Rock)	two dogs								vital organs (c)
32. 4/21/78 Bentonville, AR (NW corner)	calf								see note (y) organs removed
---Apr. 26-June 21, Elsberry, MO, 5 cows--- see note (z)									
33. 5/1/78 Little Fluck, AR	calf		x (n.s.)	x					
34. 5/8/78(*) Little R., KS	steer steer	x x (n.s.)				x x			
35. 6/8/78 Elsberry, MO (St. Louis)	calf (fem.)	x(R)	x(R)	x				x	(b)
36. 6/12/78 Las Vegas, NM	mare	x(L)	x(L)		x	x		x	
37. 5/15/78 Dulce, NM	cow				x			x	
38. 7/7/78 Prairie Grove, AR	calf			x					organs removed
39. 7/28/78 Hereford, TX (Amarillo)	mare				x			x (teats)	(b)
40. 8/4/78 Dixon, MO	heifer	x(L)				x			(b)
41. 8/16/78 Cave Spgs., AR (NW corner)	calf		x(R)	x		x			(a) (c)
42. 8/31/78 Pecos, CO (Durango)	calf		x(L)	x	x	x			(b)
43. 9/8/78 Durango, CO	cow			x					(a)
44. 9/17/78 Roderia, N.M.	bull		x(LR)	x	x	x			(b)

- a. Blood drained
b. "Surgical" incisions
c. Overt evidence of cultism
d. Black animal

y. The Manakin Gazette, Va., May 25, 1978, reported that nine white-faced cows had been mutilated at a state farm on the outskirts of Richmond in a 3-week period of late March and early April. Their ears, udders, sex organs, and anal tracts had been mutilated and autopsies showed severe hemorrhaging in heart, rib cage, and brain. There were no tracks or signs of struggle around the carcasses. On the nights of the mutilations "unidentified lights in the sky" were reported, some by guards at the state farm.

z. In late June St. Louis newspapers reported that between April 26 and June 21, 1978, five cow mutilations were reported in the vicinity of Elsberry, Mo. (Case 35 is one example.) One cow had an ear severed; three had udders, sex organs, and rectums cut "in a perfect circle"; the sex organs and rectum of a steer had been cut around and removed. Blinking lights in the sky since April 26 are mentioned, and townsfolk are said to think aliens are involved.

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THEORIES, HYPOTHESES, AND SPECULATIONS ON THE ORIGINS OF UFOS*



J. RICHARD GREENWELL



Numerous hypotheses have been advanced to explain UFO reports. They can be divided into two major categories, the first advocating conventional explanations (involving no purposeful intelligence), and the second advocating unconventional explanations (involving purposeful intelligence).

The conventional category includes such mundane explanations as aircraft, balloons, birds, and planets, and may also encompass more imaginative possibilities, such as ball lightning, swamp gas, and insect swarms. These are known as identified flying objects (IFOs). Hoaxes and hallucinations also come under this conventional category.

The unconventional category is composed of eight major "theories," although there is little to warrant the designation of "theory"; a more appropriate label would be "speculation," or, in some cases, "hypothesis." These eight "theories" are: 1) the Secret Weapon Theory; 2) the Hollow Earth Theory; 3) the Underwater Civilization Theory; 4) the Space Animal Theory; 5) the Extraterrestrial Hypothesis; 6) the Time Travel Theory; 7) the Ultraterrestrial Theory; and 8) the Psychic Projection Theory. The likelihood of each theory is assessed below.

THE SECRET WEAPON THEORY: This theory, which was more popular in the 1950s, refers to advanced technological flying devices constructed by the U.S. government or some foreign power. The proposition has some very serious problems. First, UFOs were reported soon after World War II, when military jet aircraft were barely operational. Had the U.S. had operational "saucers" capable of the performance described, it would not have expended the hundreds of billions of dollars that it has since that time in the development of alternate and less efficient military weapons systems.

Secondly, even if such craft had been experimentally tested, or even operationally deployed, they would not have been permitted to approach civilian airports, urban centers, and all the everyday places where UFOs are reported. Also, they would not have been deployed to the dozens of countries from where UFO reports have come. On the other hand, such craft would very probably have been revealed to the world as a major technological breakthrough, and as a warning to all potential adversaries. The political advantages of the latter would have been enormous.

A third problem would be the question of security. The development and operation of such craft would have involved many thousands of persons over a long period of time, and it is almost inconceivable that the secret would not have eventually surfaced. These same arguments can, of course, be applied to a Soviet secret weapon, or one from any other country. A British/Canadian secret weapon theory, for example,

was once in vogue, and one organization has been promoting a Nazi secret weapon theory, interwoven with Adolf Hitler's possible survival and escape from Berlin. The organization has published a book supporting this claim, and also makes available other Nazi-oriented books, posters, bumper-stickers, and tapes (such as "Beautiful Nazi Songs and Marches," "Songs of the Brownshirts," "Dr. Goebbels and the Third Reich," and "Adolph Hitler Speaks to the Reichstag"). As with all other "secret weapon" theories, it is difficult to imagine how such craft could have been operated for over 30 years without political utilization or without the truth emerging.

THE HOLLOW EARTH THEORY: Perhaps the most ingenious of all, the original Hollow Earth Theory was advanced by several writers early in the century, but was later linked to UFOs and popularized by Ray Palmer and Dr. Raymond Bernard. In the early 1960s, Bernard claimed that the Earth was actually a hollow sphere, with two openings at the poles, and that flying saucers belonged to a secret civilization living inside of the Earth (Bernard, 1969). The Theory supposedly gained support from observations made during Admiral Richard E. Byrd's arctic and antarctic expeditions. However, Dr. Laurence M. Gould, second in command during Byrd's first antarctic expedition, denies any such observations or discoveries. Dr. Gould, a distinguished University of Arizona geologist, informed the writer that he discussed the matter with Admiral Byrd several times prior to the latter's death in 1957, and both were amazed at the observations attributed to them. When he was President of the American Association for the Advancement of Science (AAAS), in 1953, Dr. Gould considered but finally declined an invitation to address the Hollow Earth Society then meeting in Tucson, and believes that, had he accepted the invitation, it would have been promoted endlessly as an endorsement of the Hollow Earth Theory.

The Hollow Earth Theory has also been examined by Dr. John S. Derr, a professional seismologist, formerly with the Viking Mars lander project at Martin Marietta Corp., and now with the U.S. Geological Survey (Derr, 1970). Dr. Derr discussed several types of geodetic and seismological data which clearly demonstrated that the Earth is not, and cannot be, hollow. Artificial satellite perturbations, for example, show not only that the Earth is solid, but that its mass is concentrated toward its center, contrary to that "predicted" by the Hollow Earth Theory. He also presented seismological data concerning the free oscillations of the Earth and the velocity of compressional and shear waves in the Earth following earthquakes.

The Dr. Derr analysis, plus other less sophisticated but more obvious evidence (such as the fact that numerous U.S. and Soviet satellites, which continually fly over the poles, have not, apparently, photographed the openings) leaves little doubt that UFOs, whatever they are, do not originate from a civilization in a hollow earth.

THE UNDERWATER CIVILIZATION THEORY: Reports of unknown objects entering or leaving large bodies of water (or proceeding through them) have been made from time to time, and have been labelled unidentified submarine objects (USOs). Numerous theorists have consequently speculated that secret UFO bases might be located on the ocean beds, far from man's activities and possible detection. By moving underwater,

UFOs would have access to all continents and, by proceeding up major rivers and tributaries, could reach many inland locations without risking detection by atmospheric flight.

Vehicles capable of interstellar flight, some proponents of the extraterrestrial hypothesis point out, would certainly be able to withstand the pressures and stresses of deep oceanic environments. This point has some validity, and it can also be stated that some of the most remote areas of the planet are located in parts of the southern Pacific and Indian Oceans, providing easy access from the atmosphere with minimum chance of visual or electronic detection.

At the same time, it could be asked why the UFO operators go to such lengths to remain unobserved, only to display their vehicles so blatantly in such populated areas as the U.S. and Europe.

One of the proponents of the underwater theory was the late naturalist Ivan T. Sanderson, who not only proposed that an extraterrestrial civilization could be using the ocean depths, but that a native civilization, one having evolved underwater long before man, could also be doing so (Sanderson, 1970). He concluded, in fact, that "it is likely that both suggestions apply." Although he provided no sources or references, Sanderson stated that over 50% of all UFO reports concerned objects over, coming from, or going toward (or into) bodies of water.

The Underwater Civilization Theory, like the Hollow Earth Theory, addresses the question of the possible location of UFO operational centers. As such, it is not altogether unreasonable, but it provides no real answer to the question of UFO origin.

THE SPACE ANIMAL THEORY: One of the least popular of all "unconventional" theories, the Space Animal Theory was first brought to public attention, curiously enough, by the U.S. Air Force during its Project Sign activity in the late 1940s. The Project "Saucer" (Sign was then still a classified code name) press release of April 27, 1949, admitted that the idea had been "remotely considered," and that many UFOs "acted more like animals than anything else." The Air Force concluded that few such reports were reliable. The concept was also contained in the final Project Sign Technical Report of February, 1949 (declassified in 1961).

Trevor James Constable (writing under the pen name of Trevor James) advocated a space animal explanation for UFOs in the late 1950s (James, 1958), and no other than Kenneth Arnold, the man whose sighting opened the UFO era (and who was responsible for coining the label "flying saucer"), concluded that UFOs "...are groups and masses of living organisms that are as much a part of our atmosphere and space as the life we find in the oceans" (Arnold, 1962).

Naturalist Ivan T. Sanderson again addressed the question, and many others, in the mid-1960s, concluding that there was "...nothing illogical, irrational, or even improbable about it. In fact, it is so probable that it must be given first rank in consideration of the question, 'What could UAOs [unexplained aerial objects] be?'" (Sanderson,

1967). Vincent H. Gaddis also addressed the topic, attributing the original idea to a John P. Bessor, who had sent it to the Air Force the month following Arnold's classic 1947 sighting (Gaddis, 1967). Gaddis discussed the writings on the subject by Austrian Countess Zoe Wassilko-Serecki, and John Cage, a New Jersey inventor, and concluded that "...the time will come when one or more of these entities will be caught, weighed, measured, and exhibited."

Trevor James Constable again wrote about space animals in the 1970s, this time in more detail (Constable, 1976, 1978). He postulated that the UFO-space animals "...are amoebalike life forms existing in the plasma state. They are not solid, liquid, or gas. Rather, they exist in the fourth state of matter -- plasma -- as living heat-substance at the upper border of physical nature." He also believed that they are of low intelligence, and, because they remain in the infrared part of the electromagnetic spectrum, usually invisible. He concluded that they had "...deeply confused UFO research."

Although life may be found in the most unlikely places and under the harshest of conditions on the surface of the planet, it is doubtful that biological forms could evolve in space or even in the upper regions of the atmosphere, where exposure to cosmic rays and other radiations, such as those originating from solar flares, would be maximized. The absence of oxygen for carbon-based life would also rule out biological space animals, and the possibility of life existing in a plasma state is, at best, speculative.

The Space Animal Theory has never captured the public imagination, and it has not been seriously considered by most UFO researchers.

THE EXTRATERRESTRIAL HYPOTHESIS (ETH): By far the most popular "theory" concerning the origin of UFOs, the ETH is also the one that, over the years, has aroused the most emotion and controversy. It is based on the assumption that one or more civilizations from outer space, far in advance of our own, have mastered interstellar spaceflight and have had the human race under systematic observation since at least 1947. Some see a long-term involvement by the extraterrestrials, and propose that they have been watching over man, and perhaps even controlling his physical and cultural development for millenia, thus linking the ETH to the ancient astronaut concept.

The main problem with the ETH is space and time, space in the sense that the average distance between the 130 billion or more stars in our Milky Way galaxy is enormous, and time in the sense that these great distances would make interstellar voyages very long, not to mention the economic, engineering, and motivational aspects of such an enterprise.

Despite this, there is a pro-UFO movement which uncritically believes in the ETH. There is no problem with such a belief, provided it is identified as such, and is not construed as representing an empirical fact. At the same time, the idea of an extraterrestrial origin for UFOs, as a hypothesis, is quite reasonable, despite the strong feelings against the possibility by many scientists who should know better. To deny the validity of a reasonable hypothesis because of an emotional commitment to other explanatory possibilities is not consistent with operational pro-

cedures in science, regardless of how learned such individuals may be, or how persuasive their arguments may appear.

The real basis for the ETH debate, although many involved in the debate are seemingly unaware of it, is not over whether (or how much) advanced intelligence exists in the galaxy. Most scientists will agree that there are probably many such intelligent civilizations in the galaxy. It is not even over whether such civilizations have developed interstellar travel capability. The real point of debate concerns the "volume of traffic." That is, most scientists find it very difficult to accept the idea of extraterrestrial visitation on the scale implied by UFO reports; that, to them, tends to invalidate all UFO reports. In fact, if UFO sightings were not so common (say, just one good report every three or four years), perhaps more scientists would seriously consider the ETH. Interestingly, this is precisely the reverse of what many exasperated UFO proponents realize, in their attempts to "prove" the ETH by the sheer number of reports.

Despite intensive research by many individuals, scientific bodies, and federal agencies for over three decades, no proof of extraterrestrial visitation has been produced. Such an idea, therefore, must remain as only a viable and intriguing hypothesis, very difficult to test, and frustrating to debate.

THE TIME TRAVEL THEORY: Like most UFO "theories," the Time Travel Theory lacks any empirical supporting evidence; contrary to what one might expect, however, it is probably the least popular theory in circulation.

The theory is based on the premise that man will advance to such a high technological level in the next few hundred years that, in learning how to control certain forces of nature, it will be within his ability to manipulate the barriers of time and space, and "return" to our present time, or any other he wishes. It has to be admitted that there are certain astrophysical phenomena currently being studied (i.e. quasars, black holes) which are not properly understood, and there is every indication that some fundamental natural processes in the Universe have yet to be identified, described, and incorporated into our framework of knowledge.

Nevertheless, the only hints of a time travel basis for UFOs are in the behavioral and morphological descriptions of occupants sometimes reported to pilot them. The behavioral component refers to reports that such occupants generally avoid contact, or at least do not go beyond an informal communication with the witness/es. This "policy of non-interference" would seem to be more appropriate for a society visiting its own past, which has already "happened," than for an interstellar-travelling society, which might be anxious to establish formal links with new civilizations.

The morphological component involves the biological feature of neotony, a characteristic in which infantile features are retained in the adult form. A neotenous trend is evident in the primates, particularly in man (the result is a longer childhood and the acquisition of knowledge and values by children), and the trend will theoretically continue in the

future. This implies that human adults, at some future time, could look more like today's children. Curiously, UFO occupants are described as small and childlike, with large heads relative to their bodies. Both of these speculative forms of evidence must rely, of course, on the authenticity and reliability of UFO reports involving "occupants."

The possibility of UFO occupants being extraterrestrial time travelers (i.e. not from our future) is not generally addressed by UFO researchers.

THE ULTRATERRESTRIAL THEORY: In the late 1960s, a number of UFO authorities became disenchanted with the extraterrestrial hypothesis, which they now consider antiquated. Impatient with the lack of "contact" over the years, these individuals have moved on to accept a more esoteric concept, that involving "ultraterrestrials" in a "parallel universe." These interdimensional beings are thought to share our own space, but at a different "vibratory level" of existence, and that time may have no meaning for them. John A. Keel, one of the major thinkers in the area, believes that UFOs are "nothing more than transmogrifications tailoring themselves to our abilities to understand..." and that the ultraterrestrials "...are somehow able to manipulate the electrical circuits of the human mind" (Keel, 1970).

There are so many variations of this "theory," and each variation has such loose definitional parameters, that it is difficult to describe in a systematic way. Some authorities, like Dr. Jacques Vallee, talk of long-term cultural control by such intelligences, involving the world's leading religious movements, miracles, angels, ghosts, fairies, poltergeists, and the like, and they interpret UFOs as another (but more modern) manifestation of the same phenomena. Such beliefs are not for all, however. According to Vallee, they are only for "...those few who have... graduated to a higher, clearer level of perception of the total meaning of that tenuous dream that underlies the many nightmares of human history" (Vallee, 1969). Others, like Keel, are suspicious of the ultraterrestrial's motives, and a few integrate the new theory into their previously-established religious beliefs.

As with some other areas of the "psychic sciences," the ultraterrestrial theory lacks a cohesive synthesis. This is probably because of its very (proposed) nature; there is no way to go about obtaining empirical evidence to support the hypothesis. That is, there is no "observational window" one can look through to even evaluate the reasonableness of the hypothesis. The extraterrestrial hypothesis, on the other hand, does have such an "observational window." To see it, one need only step outside at night and look up; the question of whether or not extraterrestrial visitation is possible can thus be evaluated, based on the data made available through the "window," and everyone can usually agree on the data even if they do not always agree on their interpretation. Even some of the more unlikely UFO theories, such as the Hollow Earth Theory, have "observational windows," thus enabling their appropriate evaluation.

Although the Ultraterrestrial Theory has become fashionable in some European and American UFO circles, and has gathered a substantial following, it is little known among the public, and has had little effect on

public opinion.

THE PSYCHIC PROJECTION THEORY: The Psychic Projection Theory represents a modern school of thought in UFO circles which, like the Ultraterrestrial Theory, developed as a reaction to the extraterrestrial hypothesis. The theory was first outlined by Jerome Clark and Loren Coleman in the mid-1970s, based on Carl Jung's concept of the collective unconscious (Clark and Coleman, 1975).

The authors, however, have gone beyond Jungian psychology, and postulate that the collective unconscious can psychically project material forms, represented in modern times by the UFO, and that "...the UFO phenomenon has absorbed many of the ancient archetypal forms in which human beings have traditionally needed to believe and which they have sought to complete their world."

Clark and Coleman see a danger in the modern world of science disrupting man's close bond to nature, to mysticism, and to the elements, bringing him to "the brink of catastrophe." The message they see in the UFO myth is that the collective unconscious "...too long repressed, will burst free, overwhelm the world, and usher in an era of madness, superstition, and terror -- with all the socio-political accoutrements: war, anarchy, fascism," and that "...when the unconscious can no longer be contained, its liberated contents will destroy all that the conscious mind has produced: the fruits of science and technology, civilized order, and the very process of reason itself. Under the new imbalance a spiritual dark age will blanket the earth."

UFOs, then, are merely "planetary poltergeists," which are generated by the "psychic energy" of the collective unconscious (and sometimes by an individual unconscious), as were fairies, flying saucer "spacemen," and apparitions of the Virgin Mary. In some respects, the Psychic Projection Theory relies on the same kinds of evidence as the Ultraterrestrial Theory, and it is sometimes difficult to distinguish between the writings of authorities in the two schools.

The Psychic Projection Theory would have to depend, at the very least, on both the reality of Jung's collective unconscious and the reality of extrasensory perception (ESP). Jung's writings have had wide popular appeal, but experimental work has failed to demonstrate empirically that man possesses a collective unconscious. Future research may shed more light on the validity of Jung's theory.

ESP research is a continuing and active area of research by numerous psychologists, biologists, and physicists. A very bitter and emotional debate has ensued over the years concerning the validity of ESP. Some believe that the whole subject is nonsense and should not be given any serious consideration. Others claim that ESP effects have been conclusively demonstrated in repeated laboratory experiments, and that efforts should be directed more toward understanding the effects than toward attempting to convince the skeptics.

Until the matter is resolved, the Psychic Projection Theory must remain as simply a fascinating idea. Even if ESP effects (precognition, telepathy, telekinesis, etc.) were ultimately demonstrated to be authen-

tic phenomena, bringing parapsychology into the mainstream of "normal" science, there has been no indication that such "psychic energy" could actually materialize (i.e. project) objects, whether they be fairies or flying saucers.

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CRITICAL COMMENTARIES:

COMMENTS BY GEORGE O. ABELL:

I found J. Richard Greenwell's list of hypotheses on UFOs fun to read but incomplete. Let me offer some additional ideas:

THE GAEA THEORY: The Earth (Gaea) is actually a living creature, but is concerned about her future because of increasing pollution from nuclear waste, from gases and particles in her atmosphere, and from the sheer numbers of humans. Reacting to these irritants, much as a person reacts to a common respiratory infection, Gaea emits radioactive particles into space, the luminescence of which gives rise to UFOs.

HEISENBERG UNCERTAINTY THEORY: It is well known that there is an innate uncertainty, Δp , in the momentum, and an innate uncertainty, Δx , in the position of an object, related by

$$\Delta p \Delta x = h/2\pi,$$

where h is Planck's constant. Thus, UFOs could actually be mini-quasars the most likely locations of which are billions of light years off in space, but which, due to the innate uncertainty in their positions, are temporarily in the Earth's atmosphere.

NEGATIVE ATOMIC NUMBER THEORY: Each of the naturally-occurring chemical elements is made of atoms characterized by an atomic number, which is the number of protons in the nucleus of each atom of that element. Thus, hydrogen has atomic number 1, helium 2, oxygen 8, and uranium 92. From symmetry we might expect a whole new series of elements with negative atomic numbers, holes in the universe, as it were. In fact, in recent years we have learned of "coronal holes" in the sun and probably of "black holes" in space. Negative matter should be luminous, especially firey negative hydrogen of atomic number -1. Such glowing matter may well account for many UFOs.

THE BULLPHORNIUM THEORY: Astrophysicist Lawrence H. Aller has proposed that there exist everywhere subatomic particles of Bullphornium, each of which has mass $e^{-14\pi}/\alpha$. (Aller points out that the formula for the particle mass must involve e and π because all formulae do; the α is the fine-structure constant, of value $1/137$, which must be present to give all of Eddington's magic numbers; the factor 14 is the product of 2, the base of the binary number system, and 7, the perfect number.) On this hypothesis, it is predicted that UFOs are the energy released from the mutual annihilation of Bullphornium and Antibullphornium particles.

THE GRAVITATIONAL LENS THEORY: In this hypothesis, UFOs are images of remote objects (mostly quasars and pulsars) produced by the bending and focussing of light by tiny black holes in orbit in the solar system, acting as gravitational lenses.

KIRLIAN AURA THEORY: This theory proposes that UFOs are really Kirlian auras about small insects, pieces of broken twigs

and leaves, and the like floating about in the atmosphere.

THE HORON THEORY: According to modern gage theory, all forces are transmitted by the exchange of certain particles. Thus, electromagnetic forces result from the exchange of photons, the strong nuclear force by the exchange of mesons, and gravitational forces are presumed to accompany the exchange of gravitons. Therefore, the astrological forces, by which the planets affect our lives, must result from the exchange of hypothetical horons (from "horoscope"). Horons are normally invisible, but when the planets that emit them are at certain critical aspects to each other the horons interact in such a way that their waves constructively interfere -- giving rise to luminous UFOs.

THE ID THEORY: Here UFOs are postulated to be manifestations of our ids. Electromagnetic quarkian waves derive psychic energy from the deconvolution of the hemispheric palitations.

The list is only partly complete. I could go on to mention other ideas prominently found in the writings of modern metaphysicists -- my file is full of such -- including the annihilation of matter and antimatter, chronometric cosmology, minicomets, neutrino decays, seventh-dimensional time reversals, and even flares from the sunken continent of Atlantis, in the middle of the Bermuda Triangle. Some conservative scientists argue that some of these hypotheses violate known physics, but of course they only violate our laws of physics, not those that pertain to UFOs.

It is (I hope) obvious that I propose the above ideas tongue-in-cheek. Yet, in another setting each of those "hypotheses" (or at least nearly each) could be taken quite seriously and seem quite reasonable to very many people. It is not because these people are stupid, but because these "theories" (like many others in my files) are cloaked in a scientific-sounding jargon. Who among us, aware of technological advances in recent decades, can fail to be impressed by them? Many people, with little or imperfect understanding of the science underlying our technology, have become so conditioned to what must seem miraculous to them that they can believe anything. If something is presented as science, in the name of science, and in a scientific-sounding terminology, it can seem entirely reasonable.

We cannot blame people for being misled. Science has become so specialized that scientists themselves cannot usually understand the papers of other scientists outside their own narrow areas of expertise. But there is one difference: scientists, by their training, tend to be cautious about accepting strange claims as correct until they have heard substantial opinion from established experts in the fields involved. Many members of the public, on the other hand, treat the informed opinion of experts with suspicion, preferring to accept a completely unverified and unwarranted claim by one without any obvious qualifications whatsoever. Often the claimant insists

that he is an underdog bucking the "scientific establishment," and compares himself to Galileo and Darwin (forgetting or not mentioning that Galileo and Darwin were highly trained and respected experts in their own fields). The media often encourage propagation of unfounded claims because (if only) true, they would make exciting news. Moreover, we all welcome something esoteric to liven up the humdrum news of the day.

Many cases in point are to be found in UFOlogy. To be sure, thousands of people have seen things in the sky that they did not understand. There is ample evidence that many of these sightings are misunderstandings or nonrecognitions of perfectly well-understood phenomena, and many others are inaccurately described by the witnesses (up to the point, on occasion, of complete fabrication). But let us allow, for the moment, that there is a "residue" of phenomena for which prosaic explanations are not immediately obvious. It is these objects that Greenwell is referring to in his summary. The question, then, is how we are to interpret them.

Greenwell lists eight possibilities, and I gave some others above. I was not serious, but neither are most of Greenwell's listed hypotheses to be taken seriously, and many are demonstrably wrong. Although some investigators (Hynek, for example) make no claims to preconceived opinions concerning the "correct" explanation for UFOs, the overwhelming majority of lay people, and nearly all UFOlogists as well, subscribe to the extraterrestrial hypothesis (ETH). Let us, therefore, consider the ETH.

Greenwell errs in implying that scientists deny the validity of the ETH "because of an emotional commitment to other explanatory possibilities. . ." It is, I think, the average believer who has the emotional commitment to the ETH. Belief in the ETH is, of course, understandable. One of the most profound questions before science is: "Are we alone?" Most of us hope not, and practically all of us would like to know for sure. (If, in this unbelievable vast universe, we are the only reasoning beings, or worse, the only life at all, the universe must be awesomely lonely.) All scientists I know share this wonder about life elsewhere, and nearly all hope that it does, indeed, exist. How grand it would be if Viking had found evidence of biological organisms on Mars -- either living or dead! The positive evidence, of any kind, for the independent development of life on another world would be the scientific headline of the millenium.

But Disney notwithstanding, wishing does not make it so. Indeed, the more fondly a scientist wishes a given hypothesis to turn out to be right, the more careful other scientists are about assuming that his investigations are truly objective, and the more likely they are to check and double check his findings even though they, too, want the results to be correct.

Let us be clear on this point: skepticism is not narrow-mindedness; it is the blind committed belief in something that is narrow-minded. The skeptic withholds judgement until he is sure. He does not deny the hypothesis in advance; he may, however, deny that the evidence is conclusive. Greenwell does scientists a disservice by implying that they have strong feelings against the ETH. Most scientists are skeptical just because they would like very much to find the ETH to be correct, and they know it would be a discovery of absolutely paramount importance. That is why they require iron-clad evidence.

In any event, as Greenwell points out, the debate is over the interpretation of UFOs, not over the likelihood of life elsewhere in the universe. Most scientists consider it quite plausible that extraterrestrial life, and even extraterrestrial intelligence, exists although they do differ in their judgments of the probability. We can make a fairly intelligent guess of the number of planets suitable for the development of life that exists in our galaxy; many experts would put the figure at about ten billion (10^{10}). Moreover, laboratory experiments suggest that given conditions like those on the primordial earth the formation of prebiological organic compounds is almost inevitable. What is in doubt is the probability that such complex molecules (including amino acids and sugars) will form into self-replicating organisms, that such organisms will evolve to high intelligence during the age of the galaxy, that such intelligent organisms would be interested in making social contact with other intelligent species, and how long such "civilizations" might survive. The range of "reasonable" estimates of these probabilities leads to a number of currently extant communicative civilizations in our galaxy that lies in the range one (us) to one million (very few investigators put the number much higher).

If there are a million civilizations in our galaxy of about 4×10^{11} stars, it means that about one star in 400,000 has a planet with a civilization. In our part of the galaxy, there is about one star for every 10 cubic parsecs (1 parsec equals 3.26 light years). With these numbers, it works out that there is an even chance of the nearest civilization being within about 250 light years. Now, how can we know of those fellows?

Interstellar travel is not impossible. In fact, our Pioneer and Voyager space probes will eventually leave our solar system and move on into interstellar space. But they move very slowly (a few kilometers per second). Even if we could send men into space at one-tenth the speed of light (thousands of times as fast as the Apollo astronauts travelled to the moon) it would take them 2500 years to reach the nearest civilization (if there are, in fact, a million in our galaxy), and only then if we knew where it was in advance.

As is well known, according to special relativity, if one could travel at nearly the speed of light his time would pass more slowly than would time for those who remained at home, and

in principle it is possible for a space traveler to make the 500-light-year round trip to the nearest civilization in his own life-time. But even if we were to allow him to age 100 years en route, he would have to travel 98 percent the speed of light. The energy requirements to do so are almost unbelievably staggering.

Consider the following example as an illustration: Suppose we allow 10 tons (about three Ford automobiles) for the space ship to house our astronaut for 100 years of his life, and we allow another 10 tons for rocket engines and the like. At an acceleration of a comfortable 1g (earth gravity), it would take 2.3 years to reach the speed of 98 percent of light. Astronomer Sebastian von Hoerner has pointed out that the energy would have to come from the complete annihilation of matter and antimatter, which we do not know how to accomplish on a large scale. But even if we could solve that problem, it would still require the equivalent of 40 million annihilation plants of 15 million watts each to supply the energy, which would then have to be transmitted backward (for propulsion). That transmission would need the equivalent of 6000 million transmitters of 100 thousand watts each, and all of the annihilators and transmitters would have to work with perfect efficiency, and the whole works would have to be contained within ten tons.

Just to accelerate 20 tons once to 98 percent the speed of light, not counting stopping at the destination, starting up again to come back, and finally decelerating to land on earth, would take a total of 4×10^{29} ergs of energy -- roughly the amount of energy the entire human race uses worldwide (at the present rate) for 200 years. To reach that nearest civilization and return in a practical time (say, a few years or at most a few tens of years) would require even higher speeds and far more energy. We do not, therefore, expect that we shall soon be engaging in interstellar travel at relativistic speeds to take account of the slowing of time due to the effects of special relativity.

We can, of course, travel to the stars at much lower speeds, and with more reasonable energy demands, if we are willing to take thousands of years to do so. Unless we learn how to freeze ourselves en route, this would probably mean a trip lasting many generations, and I am not sure how likely we are to commit our future generations to a trip to an unknown destination, not knowing what they would find there. At least, though, it is possible.

Now suppose that there are not only a million civilizations in our galaxy (a sort of upper limit to informed guesses), but that each of them (unlike us) has solved the problem of interstellar travel -- either by having very long lives or by tapping completely unknown and unimagined energy sources. Even under these extreme assumptions, can we account for UFOs as being spaceships from those civilizations?

Under our assumptions, there should be about 1000 civilizations within 3000 light years. But there are about 400 million stars within that same distance. Now we can imagine no way that even one of those 1000 potential civilizations could know about us. We are, to be sure, sending out signals which an advanced technology could detect, in the form of radio waves from our commercial radio and television. But we have been doing so only for a half-century, and even our early Amos 'n Andy programs are now only 50 light years away. They have not had time to reach the nearest civilization, let alone give them time to dispatch spaceships to check us out.

No, the only visiting spaceships we would expect would be those out scouting at random. Actually, they would not have to scout entirely at random; each civilization could elect to send ships to those other stars most likely to have habitable planets. That narrows the search because within that 3000-light-year radius, there are only about ten million such stars. Thus, if each of the 1000 civilizations launched one ship to a likely star, there would be about one chance in 10,000 that one would be stopping by our way. But the hundreds to thousands of reports of UFOs each year can hardly be accounted for with one alien spaceship. Suppose we were to have 1000 alien visits each year. That would require that each potential civilization launch ten million spaceships annually, sort of at random, toward solar-type stars. That's 25,000 launchings per day per civilization, or about one every three seconds. And that's only if the number of galactic civilizations is near the maximum possible, and that each of them possesses far, far higher technology than we can even imagine.

None of the above is impossible. But many people are under the impression that because the possibilities for extraterrestrial life are so great throughout this huge universe, it is reasonable to suppose that we can be visited by thousands of alien spaceships each year. Yet, when we put the actual numbers into the calculation we find that even if our galaxy is literally rampant with civilizations with the capability of interstellar travel and with energy sources we cannot even imagine, even then the odds are way, way against our having been visited even once each year; indeed, we would expect (under the most liberal assumptions, remember) only about one visit each 10,000 years.

Perhaps we have been visited once -- or twice -- during recorded history, and that all those many thousands of other UFOs have some other explanation. But if we can find ways to explain away many thousands of UFO cases, it does not stretch the imagination to suppose we might explain away an additional one or two.

It is for this reason that most of my colleagues and I are highly skeptical of the ETH for UFOs, not because we think there are not intelligent civilizations elsewhere, and certainly not because learning about one -- even one! -- would be uncomfortable. On the contrary -- I repeat -- it would be the most exciting development in the history of science!

And just because of that extremely remote possibility, I am willing to look at hard evidence. I have, in fact, spent a little time working with a small group organized by Peter Sturrock that tries to check out anything that holds promise of providing useful hard evidence. My time is very limited for such investigation, but I have been involved in three cases, one of which was completely explained in prosaic terms, and for the other two we found plausible explanations. To my knowledge, at this time there is no hard evidence that even strongly suggests, let alone requires, an intelligent extraterrestrial origin of UFOs. Much as I wish it were so, there is, alas, no reason to think it is so, and many reasons for not expecting it to be so, as I have explained above.

All things considered, I find the ETH the least likely of serious hypotheses for the origin of UFOs. Frankly, I think it is exceedingly unlikely that we shall ever learn anything about extraterrestrial life from investigating UFOs, although we may learn something of other phenomena, including human psychology. But then is there no hope of ever knowing if we are alone?

I think there is hope, and serious astronomers and physicists have considered the question very carefully. A full day's symposium was devoted to the subject at the Montreal meeting of the International Astronomical Union in August, 1979, and NASA has sponsored an ongoing study of this grand question of life in the universe. Indeed, it was one of the principal motivations for the Viking **Mars mission**!

One real way that civilizations can communicate with each other (if they exist and wish to) is by radio. Today we have the technology to transmit radio messages across the galaxy with sufficient strength that we could detect those same signals (at their destinations) with equipment like that we now possess. Both the United States and the Soviet Union have conducted radio surveys for intelligently coded signals for years. So far these surveys have been without success, but we have scratched too meager a part of the surface to have had any realistic hope for success yet. But very large-scale surveys have been proposed, with vast arrays of radio telescopes. The best known of these is Project Cyclops, proposed a few years ago by a NASA committee headed by Barnard Oliver.

I hope that some day Cyclops, or its equivalent, will be built. Even if we never detect another civilization, it will have been a shame not to have tried by a procedure that has a realistic, if small, chance of success. In any event, we are certain to learn a few things, and someday, hopefully, we will discover our comrades in the galaxy. Let us look to these grand projects, well thought out according to the best information at our disposal, and not pin our hopes on shoddy evidence, hearsay, and wishful thinking.

COMMENTS BY JEROME CLARK:

The major problem with UFO theories, speculations, hypotheses or whatever it is one wants to call them is that ufology has yet to produce one that is even interesting. Most UFO theories are just plain dumb; those few that aren't are nonetheless pedestrian, so much so that their effect is to trivialize the UFO problem rather than to clarify it. The closest thing to an exception is -- or, rather, was -- Jacques Vallee's Control System hypothesis (Passport to Magonia, 1969; The Invisible College, 1975), at least when it was sufficiently vague to allow one to read almost anything into it. But when Vallee finally got around to saying precisely what he meant -- that the Illuminati or some similarly sinister outfit was behind it all -- well, chalk one up for the wild blue yonder.

For the first time in many years the extraterrestrial hypothesis has started to make some sense to me, even if virtually every treatment of it in the literature (save for Aimé Michel's, plus Vallee's of the Anatomy of a Phenomenon period) has been inane, as if the writers had, unconsciously or otherwise, drawn on some of the more hackneyed plots of science fiction stories they had read as unusually impressionable children. Nonetheless, for all its problems (the chief of them, as Greenwell indicates, the overabundance of sightings), the extraterrestrial hypothesis is still the best bet among the "unconventional" UFO theories (though some of the theories suggested by certain would-be debunkers are so extreme and improbable that Greenwell is certainly being excessively generous when he calls them "conventional"). But surely, if the extraterrestrials are here, they are not here in quite the way that the Donald Keyholes and the Stanton Friedmans have been telling us they are.

One of the several logical crimes of the ETH is its irritating habit of anthropomorphizing presumed alien visitors, who become a lot less intimidating (and a lot less interesting) when they are depicted as mere eccentric versions of ourselves. As if that were not bad enough, a few years ago some of us who rejected the ETH took anthropocentric speculation one step further and suggested that maybe the UFOs weren't just like us, they were us. In Flying Saucers Jung had already outlined the allegedly profound symbolic content of flying saucer visions; all the rest of us had to do was to account for the occasional physical effects associated with UFO appearances. Whammo!-- "planetary poltergeists" fueled by the PK-energy of the collective unconscious -- and from there, alas, a dumb book, The Unidentified (1975), which I co-wrote and to which Greenwell is far too kind.

Greenwell writes, "The Psychic Projection Theory would have to depend, at the very least, on both the reality of Jung's collective unconscious and the reality of ESP." Well, actually it would take rather more than that. It would have first of all to prove that the concept of UFOs is important to the human race.

Yet, so far as I can tell, there is no evidence that the idea touches any deeply responsive psychological, mystical, religious or

social chord in most human beings, or even in most UFO witnesses, contrary to a basic theoretical tenet of both Flying Saucers and The Unidentified. In fact, to all appearances UFOs and UFO reports interest people simply because they happen to be novel and exotic. They are less boring and less predictable than the other phenomena of the world, which continues to function, as well or as poorly as usual, as if UFOs weren't there at all. There appears to be no real necessity for UFOs. The fact that they're still here says something, in my opinion, about their existence as an objective, independent phenomenon.

It is only a very few people who, more deeply affected than even the most dedicated mainstream ufologist (who is likely to see UFOs as a novel phenomenon which has engaged his intellect but left his soul pretty much intact), join contactee religious sects in anticipation of the Great Extraterrestrial Revelation. But it is a simple desire for novelty, I think, that causes the bored housewife to see an advertising plane and fancy it a domed spaceship. And that sense of novelty wears thin quickly enough, even in someone who believes that once he was abducted into a UFO and examined by chrome-domed humanoids. (I know that sounds incredible but I have met a number of such persons in my career as a UFO investigator. There are exceptions, naturally, usually those whose purported experiences have caused them to become media celebrities.)

Maybe the housewife and the abductee feel, at least in their more reflective moments, that they briefly interacted with the cosmos; but so what? This is life, not 2001: A Space Odyssey, and it goes on, memories fade, and other novelties -- a neighbor's escapades, a car accident, a shiny new possession -- claim the attention. These may be less interesting but they are about as psychologically or metaphysically fulfilling.

It takes some considerable imagination (and The Unidentified is nothing if not considerably imaginative) to see UFO encounters, whether they be routine or exotic, as archetypal dramas. Even Jung finally realized the futility of it all and in his last chapter, the one that both skeptics and "New Ufologists" customarily and conveniently pretend isn't there, conceded that UFOs are probably physical craft, since no other explanation, including the one to which he had devoted all the previous chapters, really made sense. He specifically rejected "planetary poltergeist" types of speculations. He also acknowledged that his psychological theory did not take into account any but circular UFOs whose shape, he concluded from a study of a handful of dreams and images, symbolized a kind of psychological integration. Cigar-shaped objects, triangles, crescents and all the rest -- well, they were nothing but...UFOs. And so we're back where we started from.

There is some slight evidence that collective PK effects may occur, but even if they do (a big if, of course) and even if they create UFOs and related phenomena (an even bigger if but still an interesting idea), I see no reason to force-fit them into a Jungian framework. Human beings may use their imaginative faculties to keep from being bored, but I doubt that they have any deep uncontrollable, unconscious impulse to populate the heavens with elusive flying objects, which

have even less spiritual sustenance to offer than ghosts do. And ghosts, if you accept the popular interpretation, have a message of paramount importance to every living person; they tell us that we don't have to worry about dying because we'll still be around after we've shuffled off the mortal coil. But that hasn't kept the ranks of the Spiritualists from dwindling even in this insecure age -- and I'm willing to wager that the flying saucer contactee religion claims even fewer adherents.

COMMENTS BY DANIEL COHEN:

I have a few minor additions and quibbles with the Greenwell piece, and one major point. First the small stuff.

The hollow earth theory became popular in the U.S. through the efforts of John Cleves Symmes in the early 19th century. The first man to link the hollow earth and flying saucers was Ray Palmer. Palmer had already pushed the hollow earth in a series of fact/fiction pieces known collectively as the "Great Shaver Mystery" that he had published in Amazing Stories back in the early '40s.

Greenwell speaks of Ray Palmer and Dr. Raymond Bernard. I suspect that Ray Palmer was Dr. Raymond Bernard. No one to my knowledge, has ever met the "real" Dr. Bernard, or knows anything about him. Anyway, whoever Dr. Bernard is or was he sure wrote and thought a lot like Palmer, and he quoted a lot of Palmer's material too.

Greenwell has convinced me that there is no hole in the pole, but I doubt if his arguments will persuade a real hollow earther. They know that Admiral Byrd discovered the hole in the pole and that the government has been covering up the discovery ever since. I have received many letters from conspiratologists denouncing the coverup.

Ivan Sanderson first supported the space animal theory in his book Uninvited Visitors published in 1967. Neither the book nor the theory proved to be very popular. By 1970 and the book Invisible Residents he was back with a new, or at least another theory. This was the one about UFOs coming from an undersea civilization. That theory and the book about it had a far wider audience.

Greenwell has failed to include my favorite Sanderson quote. It is about his theoretical undersea civilization. Sanderson suggested that they may be "overcivilized and quite mad." They have so much technology at their command that they "could live anywhere or everywhere, and move about instantly, or faster, anywhere throughout space and/or time." You can't prove it, but you can't argue with it either.

I object strongly to Greenwell's statement that "the real point of debate" over the ETH is the "volume of traffic." "The real point of debate" is over the quality of the evidence. Also he implies a greater consensus on the existence of intelligent civilizations in the galaxy than truly exists in the scientific community today. Indeed, there is currently a reaction against the "we are not alone" theories.

The ultraterrestrial and other exotic UFO ideas were first discussed in the 1950s. John Keel has now moved well beyond the simple "ultraterrestrial" explanation. He now blames the "super-spectrum" which is "the only reality." I don't know what he means either, but that is what he says.

Greenwell fails to note that monsters as well as fairies and the Virgin Mary are often mentioned in UFO theory. There are numerous reports of Bigfoot sightings in connection with UFOs.

But my primary objection to Greenwell's piece is that it touches far too lightly on the reasons that many of the leading lights of Ufology, Vallee, Keel and Hynek for example, have migrated from the nuts and bolts spaceship theories of the 50s to more esoteric explanations. It is not merely the lack of contact that has disappointed them, it is the total lack of concrete evidence of any kind.

These Ufologists find themselves in a "save the phenomena" situation. Without tangible evidence they have come to accept theories in which tangible evidence is unnecessary. Indeed, some have conclusions in which the very lack of evidence is put forth as the best evidence. Keel is particularly adept at this maneuver.

It is a migration of theory which has taken place before, in magic, for example, which moved from the very practical business of finding lost treasures and raising storms to the current stress on mustering "psychic" forces. Psychical research started as a quite straightforward attempt to communicate with the dead, and failing to accomplish that has moved into realms where small and perhaps chance effects can keep spiritual hopes alive.

Most religions I think, have made the same sort of move. Christianity began with the very practical idea that the world was going to come to an end and the Kingdom of God would be established at almost any moment. But Christians have managed to overcome that little miscalculation, though there are still a few who say the end is just around the corner, just as there are those who expect the great spaceship landing to take place at any moment.

COMMENTS BY WILLIAM R. CORLISS:

The solicitation of comments on the Greenwell manuscript provides the opportunity to make two rarely expressed points about UFO data and theories. The first is that UFO theorists, including Greenwell, and UFO data accumulators treat UFOs as a well-isolated phenomenon. Some admit to a psychic tinge in the data, but few acknowledge that UFOs occupy only a small portion of a broad, continuous spectrum of anomalies. Some of these anomalies--transient lunar phenomena, for example--are almost in the grasp of contemporary science. Others, such as UFOs, sea serpents, and ESP, are so far from the mainstream of science that the data are ridiculed and frequently swept under the rug. Between the extremes are thousands of other anomalistic phenomena in all disciplines, ranging from New England megaliths to automatic writing. In all fields of science, the anomalies span the scale from zero anomaly units to very high on the scale. I would go so far as to say that every phenomenon has some anomalistic content. It is merely a matter of not shearing off the wild points and looking hard for the fine structure, as in the continuing string of surprises in modern particle physics. With UFOs, of course, the anomalousness of the data is all too obvious.

My second point is that Greenwell's paper and most UFO literature for that matter do not admit that gross, often closely related anomalies--at least as disturbing as UFOs--prevail in all disciplines. Furthermore, the small bands of researchers clustering around some of the major anomalies (ESPers, Sasquatchers, Catastrophists, etc.) are so entranced with their own deviations from prevailing paradigms that they will not acknowledge other shaky structures. ESPers and UFOers are contemptuous of the Creationists, who actually have fine collections of anomalies. But the Creationists snort at UFOs and Ancient Astronauts. All are prisoners of hypotheses that may be rotten to the core. Even many geological Catastrophists cast stones at the hundreds of cases reported in the reputable literature of live toads being found in solid rocks. Wait now, if you have just grimaced and pooh-poohed these hapless toads, you are a proven anomaly snob. They are just as possible and just as impossible as UFOs. The toad-in-the-solid-rock phenomenon is backed by ample testimony from respected people; there are even physical traces in the form of toad-shaped cavities in the rocks. The whole idea is just too impossible isn't it? The living toads violate all the laws of geology and biology. And beyond the toads rise rank after rank of other fascinating anomalies. Too bad we all have our own private brands of myopia.

Can we really get to the heart of the UFO phenomenon without recognizing that it is part of an indivisible spectrum of other anomalies that differ only in degree, flavor, strangeness, and outrageousness?

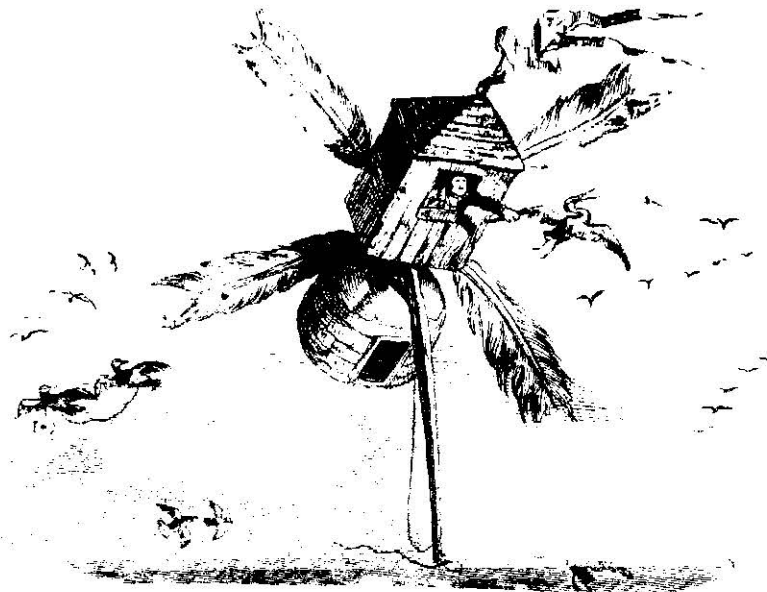
COMMENTS BY JOHN S. DERR:

I think Greenwell has done a fine job summarizing the "theories, hypotheses, and speculations" from my point of view as a physical scientist. The article's purpose is to be a short summary, and I think this goal is well met. Those with vested interests in the other theories may have some criticisms, but I feel that he has certainly been fair to me. If anything, I would tend to downgrade the hollow-earth theory to a wild idea, undeserving of the press it has received, and concentrate instead on his last four subjects: extraterrestrial, time travel, ultraterrestrial, and psychic projection. These are worthy of extensive discussion, although I would not expect any significant progress toward understanding the UFO phenomenon to come from it.

Rather, if one seeks progress toward understanding, I would steer more attention toward examination of Persinger's (1979) ideas on transient geophysical phenomena, in which I see great merit. Greenwell puts these into the conventional category, but I think that does the ideas an injustice: although Persinger's suggestions are the stuff of physical and behavioral science, they are hardly conventional! I believe that more than just a few of the hard core of sightings and close encounters can be explained by this geophysical/behavioral model, and that separating out those cases might focus some light on one of the so-called "unconventional" explanations as possibly having more merit than the others. However, much work remains to be done to validate the Persinger model, and until then, it belongs to the class of "imaginative possibilities" under the "conventional" category.

REFERENCE

Persinger, Michael (1979). "Possible infrequent geophysical sources of close UFO encounters: expected physical and behavioral-biological effects." In R.F. Haines ed., UFO Phenomena and the Behavioral Scientist, Scarecrow Press, Metuchen, NJ



COMMENTS BY CHARLES FAIR:

Among the eight theories of UFO's concisely reviewed and evaluated by Greenwell, there is not a one that he or I or anyone even moderately acquainted with the facts and methods of science could be expected to take seriously. Some - such as the Hollow Earth Theory - are simply updatings of much older nonsense. (Cotton Mather and, I believe, the astronomer Halley, were Hollow Earthers.)

The Secret Weapon Theory, besides being implausible for reasons Greenwell points out, has clear overtones of paranoia. Given the actual missile-capability of the superpowers, why should people work themselves up about hovercraft which (as Phil Klass once remarked) seem to delight in buzzing barnyards or causing radar flaps at busy airports? The pilots of these craft must be as idiotic as the people who believe in them. And in that case, why haven't any been shot down?

This last is a key question. Aside from a controversial bit of magnesium supposedly recovered from a UFO that blew up in Ubatuba, Brazil, there is no evidence for the existence of UFO's. No matter where they might have come from - the ocean depths, outer space, or from some "other dimension" - it is inconceivable that none has ever crashed or had a mid-air collision (the traffic around our airports being what it is).

The Underwater Theory raises grave difficulties. If a civilization of beings like Shaver's Deros exists down there, by what kind of technology do they make their flying-machines? If they are, themselves, flying organisms (Sanderson's idea), what is their method of propulsion? We should certainly learn about it, since the UFO reports I've read usually included mention of incredibly swift maneuvers. None of the ocean exploration now going on has turned up anything that moves very fast; on the contrary; life in the depths seems to loaf along, as you would expect, the inhabitants being poikilothermic and living in a cold dark undesirable niche.

In The New Nonsense (Simon and Schuster, 1974^{*}) I reviewed various astronomical arguments against the possibility that we were being visited by space-ships from some other solar system.

*Don't try to find it. It was published, I think, in secret. For those interested, I have a spare copy or two. When I told the late Donald Menzel what the book was about he said: "It will never sell. You should take the other side and write nonsense." The book he was then writing did not sell either. In fact I've never heard of it since.

The essence of these was that we live too far out and too far above the plane of our galaxy to be accessible. And even if one assumes that there may be habitable planets as near by as Tau Ceti or Epsilon Eridani (about 11 light years away), and that on these, superhumans may have evolved, what are the odds that their evolution just happened to be synchronous with ours? Very poor I should think, given the billions of years the whole system has probably been in existence, and the enormous difference in the rate of evolution which could be produced by a slightly higher or lower mean annual temperature (to name only one of thousands of variables) on these planets as compared to ours. Moreover, the life-expectancy of high civilizations - to judge from our own - may be rather short. So we are talking about the possible simultaneity, as it were, of two sparks in the vast stretches of cosmic time. It is significant, in this connection, that radio astronomy has so far picked up nothing suggesting a signal. Even if it eventually does so, the signal may come from a civilization long extinct.

One interesting feature of the UFO question is the time it has caused us to waste in refuting ideas so infantile one wonders why they even came up. Whatever it was that made Halley, and later U.S. Army Captain John Symmes, believe that the earth was a hollow spheroid open at both ends, it is incredible that in the era of modern polar exploration the idea still survives - that Admiral Byrd's associate, Dr. Gould, should have had to repeat that no hole was found down there; that Dr. Derr, a seismologist, should have had to review evidence showing that besides not being open-ended, the globe isn't hollow either.

What this says about the American public is startling. The media reportedly estimate our mental age as, on average, about twelve - a number I have never understood, since if that is our mean or median I.Q., then the normal distribution curve, compiled from figures related to the subject's chronological age, must be all wrong. Be that as it may, what reasonably intelligent and educated 12-year-old would be likely to believe the Halley-Symmes Theory? Hollow Earthers should be closer to mental age 8 - but would adults that retarded be theoretically-minded at all? The remaining possibility is that they are not retarded so much as mildly insane. Given the number of other strange cults in this country, this means that our population of the cracked runs to millions, which sounds funny but isn't.

The Other Dimension Theory cannot refer to anti-matter, since vehicles from that realm would self-destruct in ours. The theory sounds to me like the old idea of the spirit world rephrased to seem scientific.

Finally, as to the Jungian, or Psychic Projection Theory; that has the mystification that Jungians appear to love. The grain of truth it contains is ominous without being very exciting.

We live in an anxiety-ridden age, and mass education, far from taking, seems to be losing ground. People whose intellects are poorly formed and whose emotions are kept in turmoil, are naturally prone to "see things." What they see depends upon their temperament - hence the Devil and Angel Schools of UFO theorists discussed in my book. The Space People are coming to save us, or to make us slaves. One faction sees them as hovering overhead and manipulating us by "psi forces," whatever those are. They will save us from nuclear annihilation - are waiting right now, to intervene, in case the Afghanistan affair should get out of control. Or they will land and the Andromeda strain will hit us, saving them the trouble of getting rid of us with their ray-guns. It all depends on your state of mind, how you regard "them."

When a Dr. Albert Hibbs, of Caltech's Jet Propulsion Laboratory, was asked, by a UFO organization, what we should do if we received a message from outer space, he replied: "Hang up. Look what happened to the Indians." I'd be inclined to agree, but is there anyone on the line? Not so far, it seems. But then, last fall, the National Enquirer quoted two Russian scientists to the effect that ten pieces of an exploded UFO were floating around in orbit with the rest of our space junk, some of the pieces perhaps containing the remains of extra-terrestrials. So maybe the case isn't closed after all. If anyone wants to put money on that, though, I'll give him good odds.

COMMENTS BY ROBERTO FARABONE:

I carefully read J. Richard Greenwell's paper and found it quite interesting. This paper considers the most important known theories which pretend to give an explanation for UFO phenomena, describing them briefly and clearly with some short comments; the article essentially gives accurately what a lot of researchers have imagined in order to reach a solution to this controversial problem.

What I cannot understand is the reason why, in ufology, one must go on using non-orthodox methods. Today, when that argument begins to be taken into consideration by an increasing number of people involved in science, I think that the approach to this matter has to be changed.

We know it is true that the majority of backward people in the academic world refuse these studies, but that is exactly why we must fight on their field.

I really think that in no other branches of knowledge have so many hypotheses been made "a priori," trying to explain all phenomena as being relevant for ufology; all this is done without considering (or considering only marginally) the possibility of clarifying the basis of the research in the field, and also without asking for homogeneous data which could be the basis for serious research. Besides that, common criteria of scientific method are not followed: one should not explain the unknown with something not demonstrable.

As a matter of fact, all reported theories have in common the characteristics of being either partial (the conventional theories) or being mainly produced by emotional stimuli (the other theories). It is not taken into consideration that if people want to give a scientific structure to this intriguing field, it will be necessary to follow all the logical steps which bring realization to science. Therefore, for what concerns us now, our problem should not be that of creating theories following our will or preferences; in this way we risk submitting theories which are not verifiable and which cannot be falsified; this will cause a lack in the basic requirements which permit us to face an hypothesis with scientific methods.

It will therefore be necessary to go back to the conceptual economic principle, well known as Occam's razor, which pushes us to build our knowledge models utilizing always the majority of what is already known, accepted, verified, and running in fields we already know with a certain accuracy.

It is true that acting in this way we are sticking to what Kuhn calls "normal science": on the other hand, to recognize that UFO phenomena constitute an anomaly for our present scientific structure, we must first verify that it cannot be dealt with by present scientific structures.

This already important duty must be fulfilled in the near future.

COMMENTS BY LUCIUS FARISH:

I would begin by saying that I feel Richard Greenwell has done a good job of summarizing the leading UFO theories. I find myself in general agreement with him in virtually all respects.

The Hollow Earth Theory does seem to have numerous holes in it (pun intended!). While I find it difficult to credit the theory as it is generally expressed, I do feel that some of the older literature on the topic is of interest. Admiral Richard E. Byrd's supposed statements in support of the "HE" theory have been grossly distorted by numerous writers, as can be seen by anyone who will bother to check the February 1947 issue of National Geographic for Byrd's Antarctic account.

The Space Animal Theory is, as Greenwell says, one of the "least popular" of the various UFO theories. However, I do not feel that the theory should be discarded, although I suspect it could serve to explain only a very small percentage of UFO reports. It is interesting to note that the late Dr. Carl Jung apparently gave serious consideration to this concept. In the original German edition of Jung's book, Ein Moderner Mythos, he refers to UFOs as Wesenheiten, which is to say, "being" or "entities." In the English translation of his work (Flying Saucers: A Modern Myth of Things Seen in the Sky), Jung's translator, R.F.C. Hull, substitutes the words "objects" or "phenomena," thereby altering the meaning. Jung thought it possible that UFOs are "real material entities of an unknown nature, presumably coming from outer space, which perhaps have long been visible to mankind, but otherwise have no recognizable connection with the earth or its inhabitants." In other sections of the book, he compared the actions of UFOs to those of certain insects swarming in the air.

Greenwell is correct in saying that one of the major points of contention with respect to the ETH is the "volume of traffic." There are simply too many reports of UFOs for most scientists to seriously consider them as extraterrestrial craft. However, this presupposes that these same scientists know how much interplanetary or interstellar "traffic" is supposed to exist. They do not! All the theories about the vast cosmic distances and the time required to journey such distances are only relevant to our present state of knowledge-- a state which surely must be miniscule, at best, on a cosmic scale.

Another factor to be considered in any discussion of the ETH is the concept of bases. During the Vietnam War, it would have been folly for an observer in North Vietnam to suggest that the B-52s which bombed the country daily had flown from the United States that morning and flew back again when their mission was completed. So it is, I suspect, with UFOs. If no UFOs originate in our solar system (and I do not dismiss that possibility either), then one should surely consider the probability of bases on various planets and satellites of this system, including Earth itself.

The Ultraterrestrial Theory, Greenwell says, "lacks a cohesive synthesis." This is quite true. It should also be added that while the "UTH" cannot be rejected as a possible, partial "answer" for UFOs, one finds that some of the major treatments of this theory (the writings of John Keel, in particular) are filled with distortions, inaccuracies and unsupportable hypotheses. Any concept founded on such weaknesses must surely be called into question.

The Psychic Projection Theory, while an interesting concept, is totally unproven and likely to remain so. It also tends to ignore many aspects of the total UFO picture, which weakens it still further.

COMMENTS BY STANTON T. FRIEDMAN:

Greenwell's otherwise excellent article is flawed by it's inadequate treatment of the ETH. The ET origin of SOME UFOs is deducible from the data not an assumption. Some few reports, landings, radar-Visual sightings, abductions, etc., are of objects of definite size, shape, surface texture, etc. The very peculiar flight behavior and appearance TOGETHER indicate they are manufactured elsewhere than on earth.

The average distance between stars or civilizations is meaningless. For me the average distance to the 10 largest American cities is over 1800 miles, but the nearest is only 30 miles away. Zeta-1 and Zeta-2 Reticulans each have another sun-like star less than 3 light weeks away compared to our nearest possible neighbor distance of over 4 light years. Even at only 0.1c trips between Zeta-1 and Zeta-2 would take only 6 months. At 1 G acceleration it takes only 6 months to reach 0.5c. At 99.99% c it would only take 6 months pilot time to travel the distance between Earth and Zeta Reticuli (37 light years).

There is no reason to assume that any Earth Excursion Module comes here directly from another solar system and then returns there. The volume of traffic is a complete red herring. Tens of thousands of observations of American bombing planes were made by Vietnamese as a result of one aircraft carrier ("mother-ship") voyage from the USA since each of the 75 planes made many trips from the carrier--NOT from the USA. Thousands of genuine EEM sightings could result from one major interstellar flight. We may even be the least visited planet in the entire neighborhood.

Once interstellar travel is achievable (10 million years ago?), trips may be as comparatively easy as trips to the moon became within 20 years of Sputnik. Any study of technological development reveals that progress comes from doing things differently in an unpredictable way. Though the solar system is more than 4 billion years old, it was 1937 before we determined how the sun produces its energy. Yet, similar fusion processes in a properly designed rocket can eject readily available propellant particles with 10 million times as much energy as they can get in a chemical rocket. Civilizations just barely ahead of us (say only one million years) surely will have even better techniques.

That "no proof of ET visitation has been produced" may be true but is hardly relevant. Governments having crashed saucers would certainly not produce them because of the military vehicle implications and the threat to any national governing body of the earthling orientation which would result from revelation of positive proof. However, the vast amount of available evidence of the ET origin of SOME UFOs and of the Cosmic Watergate the phenomena represents do indicate "beyond a reasonable doubt" that SOME flying-saucers are ET in origin.

That aliens might be anxious to establish formal links with new civilizations seems silly. One doesn't negotiate when one has all the cards. Who speaks for planet Earth? Surely they can monitor our information transmissions and randomly sample flora and fauna without any permission from us. From an alien motivational viewpoint, one fact of interest to all neighboring advanced civilizations is that soon (less than 100 years) we will be going to the stars. Unfortunately, every new frontier for Earthlings has become a new place to do battle. From an alien viewpoint (perhaps ours as well?) we are a primitive society whose major activity is tribal warfare. Certainly they would wish to monitor us before we leave here to bother them and before we can prevent them from monitoring with our rapidly developing laser weapon technology. Monitoring and contact are two very different kinds of interactions.

The arguments for and against the ET hypothesis are spelled out in far greater detail in the following papers:

1. Friedman, Stanton T., "The Case for the ET Origin of Flying Saucers," in 1979 MUFON Symposium Proceedings, pp. 208-226. \$8. From MUFON (103 Oldtowne Rd., Seguin, TX 78155).
2. ---, "Science Fiction, Science, and UFOs," in 1977 MUFON Symposium Proceedings, pp. 137-166. \$5. From MUFON.

3. ---, "A Scientific Approach to Flying Saucer Behavior," in Thesis-Antithesis (symposium sponsored by AIAA and World Future Society, Los Angeles, 1975), pp. 22-36.

4. ---, Ufology and the Search for Extraterrestrial Intelligent Life, June 1973, 25,000 words, 73 references. \$1.25 postpaid from UFORI, POB 502, Union City, CA 94587.

5. ---, "Flying Saucers ARE Real," in Worlds Beyond. And/Or Press 1978. Pp. 200-225. Available from UFORI, \$7.

6. Dickinson, Terence, "The Zeta Reticuli Incident," 1975. 32 page full color reprint of the December 1974 ASTRONOMY magazine article and subsequently published letters by Sagan etc. Originally \$4, now only \$2.50 from UFORI.

Note Items 1, 2, 3 are available as a separate reprint from UFORI for \$3. Items 1-4 and 6 are available from UFORI for \$6. Postpaid.

COMMENTS BY ALLAN HENDRY:

There is a field of study that concerns itself with the collection of anecdotal accounts of unusual, glowing aerial objects. The descriptions encompass a broad range of shapes - cigars, spheres, rods, discs - and motions ranging from hovering near the ground to rapid departure to sudden disappearance. Sometimes these anomalous objects make noise, often they are silent. Of particular interest are those reports that link burns and damaged artifacts with the sighted forms. Some have actually been photographed, according to the witnesses.

There are obstacles, however, that stand in the path of researchers of these reports, preventing them from applying the conventional rigors of science:

*The phenomena at stake are invariably transient and elusive and cannot be transported into a laboratory environment.

*As a consequence, researchers are armed only with human testimony as data to support near-miraculous claims.

*As a further consequence, new theories are constantly being formed to account for these flying objects...all without any sense of confidence, for reasons listed by James Oberg in his recent New Scientist essay: the absence of data verification, theory testing, and burden of proof. Naturally, then, there are many scientists who continue to doubt the very existence of these alleged aerial anomalies.

By now, the reader has guessed the subject under study: BALL LIGHTNING...of course!

Punchlines aside, the point is worth noting. The problems besetting the successful formulation of theories to account for

UFO reports are shared by other fields of endeavor...with the same frustrating results.

At least ball lightning reports seem sufficiently uniform to allow them to fall under the aegis of a single scientific discipline: meteorology. Accounts of everything from "glowing orange basketballs" to "domed machines with occupants" all fall under the collective "UFO" label and make any single explanation scheme - including those of the skeptics - untenable. Worst of all, my own study of hundreds of "UFO" reports based ultimately on identifiable objects like stars, advertising planes, aircraft lights and meteors proves to me that a broad cross-section of people routinely enhance their descriptions of these "IFOs," attributing to them forms, behaviors and special effects contoured to fit pre-anticipated "flying saucer" concepts. Our ability to accept UFO testimony at face value (in the absence of valuable features like "independent witnesses") is diminished, then, as the same types of people are reporting worthy-sounding UFOs as IFOs in my case collection.

Any change in our ability to produce more meaningful theories for UFOs will have to come about in one of two ways: a change in the way the phenomenon reveals information about itself (which I personally doubt) or a change in the methods we use to investigate the sightings...methods which might not be developed for some time to come.

COMMENTS BY ELAINE HENDRY:

It has always seemed to this respondent that one of the single most important items which has stood in the way of progress in the struggling field of UFology is the UFOlogists' obsession with UFO theories. Greenwell has done a commendable job of summarizing the precepts of the various camps, but I question the underlying wisdom of such an act at this time. Instead, I feel that there is a simple and important underlying problem which might possibly negate UFOlogists' well-intentioned efforts in this direction.

This difficulty is implied by Sherlock Holmes' classic statement, "It is a capital mistake to theorize in the absence of data." By this I do not mean to imply that there exists a lack of data altogether in UFology. However, one must ask the highly significant question, "What kind of data do we have at our disposal in UFology?". The answer to this question can materially affect the outcome of our theorizing as well as perhaps even go so far as to invalidate our very ability to carry it out. One must never forget that human testimony is the rock on which UFology must stand or fall. Instrumented UFO incidents are infrequent and, unfortunately, tend to be rather ambiguous in nature. Examining witnesses' descriptions of behaviors and appearances of UFOs reveals only the inescapable fact that there is a hopeless diversity of object types and manueurs, despite the sometimes overly optimistic efforts of investigators to closely categorize the phenomena. Gross similarities may indeed exist but the differences in major details would seem to surpass even a factor

that would take into account witnesses' normal exaggeration and inability to recount all aspects of the sighting accurately. As a consequence, we immediately are checked in the process of theory creation by the unfortunate fact that, despite the degree of care taken, each body of UFO data is different from all others.

This is a very serious problem! It is an inescapable precept of the philosophy of the scientific method that there be repeatability of data. If this does not seem to be the case, then we have grave problems. Of course, there are two obvious possibilities which can resolve this difficulty. First, we can postulate that the scientific method itself is at fault for not being able to cope with a phenomenon which perhaps mutates in some non-repeatable way as time progresses. However, if we accept this, we are left with no obvious way to treat the UFO problem at all. Perhaps a new schema can be devised but at present we are left helpless. More importantly, to discard the highly useful concept of the present scientific method on the account of this phenomenon would imply that UFOlogy occupies a virtually unique position. Obviously, it is rather premature to adopt such a radical course. Rather, at this point, it seems more likely that the fault lies with the data itself.

The second possibility is that the UFO phenomenon is somehow consciously aware of our study and is deliberately introducing confusion for reasons of its own (a la Vallee's control system hypothesis). This is, of course, possible but at present seems to suffer from an absence of proof and an abundance of wishful thinking on the part of some UFOlogists.

In general, it would seem to be an excellent first approximation that it is only through organized and, alas, conventional application of the scientific method that a theory can be found.

With this in mind, let us briefly consider our data once more. It has become fashionable for UFOlogists to place a great deal of importance on witness sincerity as a determinative factor of the reality of the UFO phenomenon. Yet the work summarized in THE UFO HANDBOOK and that of others has shown that there exists a more widespread emotional context in which the concept of the UFO plays a very much stronger part than has been previously suspected or admitted. Recent experiments under hypnosis of imaginary abductees and of those persons who have misperceived a known "IFO" and yet attributed to it highly unusual activities and behaviors (including those which are supposedly only the province of "true UFOs") at least warn us to tread with great caution where people and their psyches are concerned. Unfortunately (to be brutally blunt), the majority of UFOlogists have no training whatsoever which could qualify them to be "experts" capable of commenting meaningfully on the majority of UFO experiences. Amateurs, however well-intentioned, are simply unable to deal with possibly deep psychological needs and processes. This is not to say, of course, that such people are useless to the cause. Indeed they are invaluable in the collection of data. However, UFOlogists too have emotional needs and must guard against the hazard that adherence to a pet theory could cause misinterpretation or coloring of data. Or, perhaps, a lack of familiarity

with even conventional psychological processes could bring about the mistaken literal interpretation of a datum which might be readily understandable or even commonplace to a trained psychologist. If such an assessment seems overly harsh, let it be understood that a serious review of UFOlogy's methods and directions would seem to be in order, in view of the lack of significant progress achieved in the last three decades. Perhaps being harsh at this critical time will prove to be a kindness in the future.

Insofar as the individual theories cited by Greenwell are concerned, it would seem that they all share one rather striking characteristic. Virtually all invoke the agency (in one form or another) of "forces as yet not well understood". Here enters a bit of reverse temporal provincialism in the assumption that it is necessary to describe one unknown process by another which is equally ill-understood. As a consequence, in a very real sense, (with the exception of the at least concrete hypothesis that UFOs are misperceived IFOs) none of the theories cited explain any facet of the UFO phenomenon. The existence of the agency behind each theory is hypothetical at this point. For example, if extraterrestrials or if psychic projections or if control systems exist, then they explain UFO phenomena. There are far too many "ifs" for anything useful to be gained.

One final point: a rather overlooked implication in these frequently cited UFO theory debates is that only ONE of the above theories must, by necessity, account for all sightings. How surprising it would be if the answer turned out to be that they are "all of the above"? Or "none"?

UFOlogists' obsessions with theories for UFO origins have contributed greatly to making the subject appear to be ridiculous in the eyes of the public as well as the scientific community. How easy it is to ridicule the subject as a whole by merely ridiculing the idea of spaceships! I am old-fashioned enough to believe that it is distinctly possible that no serious attention will be paid to answers even if they are found unless this attitude can be conquered and the UFO problem opened up to the investigations of the scientific community at last. A step in this direction would certainly be a de-emphasis on premature UFO theories. But then again, perhaps the resolution of the controversy might not be as satisfying as the search! As Disraeli once quipped, "Decision destroys suspense and suspense is the charm of existence."



COMMENTS BY RICHARD C. HENRY:

Greenwell summarizes those theories of UFO sightings that involve "purposeful intelligence": intelligence less than our own intelligence ("space animals"); intelligence equal to our own ("secret weapon" and "psychic projection"); and intelligence perhaps much greater than our own (all of the rest).

We have found amino acids in meteorites, and complicated carbon-based molecules in interstellar space. Fred Hoyle has recently speculated that influenza is caused by "space animals." These problems are tractable, and if UFO's are space animals, it is only a matter of time until we know it.

The "secret weapon" theory seems discredited by the passage of time. Doesn't the same argument discredit the popular idea that the government knows all about UFO's, and just isn't telling us?

"Psychic projections" is interesting, because it raises the question, "how well will we ever know ourselves?" The advance in scientific understanding of ourselves that will occur over the next decades, I am confident, will be very great, but are there logical limits to this process?

The really interesting theories are those that involve intelligences much greater than our own, because these theories raise such fundamental intellectual issues. How can the relatively stupid (us) understand the very intelligent (them)? This is a question that would be of enormous intellectual interest, even if there were no UFO reports. It is a question that societies in the past invariably have been deeply concerned about, but which today's science almost never addresses.

It was Arthur C. Clarke who noted that "sufficiently advanced technology is indistinguishable from magic": a billion-year-old civilization could surely do what it wanted with us. If "they" live inside a hollow earth, they could give the run-around to both Admiral Byrd, and Dr. John S. Derr. But would they? Obviously, I feel I can exclude the "hollow earth" theory, but why? And what can't I exclude? It is this boundary that I think is explorable, to some extent, and that has not yet been explored. Just as the strong observational suggestion (not yet proof) that there really are black holes, has led to extensive theoretical work on what black holes should look like, so, I expect, moderately convincing evidence that higher intelligences are at work in the universe would lead to a lot of thought on the subject of their probable nature and behavior. The UFO reports that exist do not seem to constitute sufficient evidence to generate any great interest in this important topic among scientists.

COMMENTS BY J. ALLEN HYNEK:

Many years ago the Princeton astronomer Henry Norris Russell published a small, well-written book, On the Origin of the Solar System. In it he first set down, meticulously and in true scholarly manner, all the well established, observed and rigorously derived facts about the solar system--its properties--which any viable theory of the system's origin must satisfy. (Distribution of angular momentum, the sense of the rotation and revolution of the planets, the co-planar system of planets and the fact that planetary satellites (with the exception of our moon) were more in the equatorial plane of the planet than in the ecliptic plane, etc. etc.)).

It was an exemplary job, but to date we still don't know the origin of the solar system except in the vaguest of terms. No theory has satisfied the Russell tabulation. This in no way, of course, invalidates Russell's efforts; it simply means that while our knowledge of what was to be explained was excellent, our ability to do so was sadly lacking. Russell demolished the theories of the day simply by demonstrating that they didn't satisfy the known properties of the solar system.

We need, in Ufology, a "Russell tabulation" of established UFO facts that any theory must satisfy to remain viable. Unfortunately, we do not have it. It isn't a matter of just setting down and organizing already well established facts; to do a "Russell tabulation" for UFOs literally means that one would have to answer in each of many thousands of instances the fundamental Ufological question: "Did the reported UFO event-details actually happen as reported?" That is, "Did the car engine actually stop when a UFO appeared?"; "Did the skin burns (or paralysis, or eye trouble, etc.) occur as a direct result of the UFO?" In short, we need a list of established and defensible-in-a-court-of-law UFO facts, or, perhaps better put, we need to know with precision the properties of the phenomenon we call the UFO.

To do so would require adequate funds for professional, full-time attention. Decades more of week-end jaunts by amateurs to "determine the facts" by cursory interrogation of the witnesses and photographing the general terrain will not provide the needed "Russell tabulation." One must, however, fight a war with the soldiers one has, and the army of emotionally motivated, well-meaning UFO investigators has done remarkably well in establishing with high probability that a real UFO phenomenon exists, one whose properties may well be as described. Very likely cars do get stopped by UFOs, and scorched circular rings are produced when UFOs land -- but "very likely" is not enough. We need to know, "Do cars get stopped by UFOs?" and be able to prove it.

Until then, discussion of theories of UFOs is likely to remain academic and a comparison of theories impossible except in the first order. One can eliminate certain theories, not on the grounds that they don't satisfy our "kitchen-tested" list of properties, but, as in the case of the Hollow Earth theory, on the grounds that the

theory itself doesn't make sense: celestial mechanics and seismology establish beyond all doubt that the earth is not hollow. Or, in the case of the Time Travel theory, one would have to show first that there is such a thing as time travel, quite apart from any UFO considerations.

Proponents of the Ultraterrestrial Theory (other dimensions, parallel realities, etc.) have some fairly visible straws in the wind to keep their theory in the ring (the body of alleged evidence for the existence of the psychic realm), and hence one cannot dismiss it as one can the Hollow Earth Theory. Indeed, in so far as a preliminary list of observed UFO properties goes, this theory, though "far out," does furnish a mechanism for making sense out of some of the weird aspects of the UFO phenomenon as no other theory does (the reported dematerialisation of a UFO in one spot and its sudden reappearance in another, the "poltergeist-like" actions often reported, the reported communication with humans by mental means, etc.).

Greenwell's paper serves a useful even if somewhat academic purpose, but the reader may get the idea from between its lines that a "UFO Russell list" is available to work with; that is, that the facts of the UFO phenomenon have been established. There is, however, much work to be done to firmly establish the reality of the panoply of things reported as true about the UFO phenomenon. It simply cannot be taken for granted that what has been reported about UFOs are indeed facts. We do not know, for certain, despite the many remarkable similar stories from around the world, that some UFOs contain occupants who can abduct and medically examine humans. The fundamental ufological question has not been answered.

Based on my long association with the problem, I am now willing to say (as I was not earlier) that the probability is high that the answer to that fundamental question is "yes". That satisfies me and provides me with incentive to carry on. But no one realizes more than I do that we really need a kitchen-tested Russell tabulation of the properties of the UFO phenomenon that are as firmly established as were the astronomical facts in the original Russell tabulation. Even if we had it, we might not get any farther than Russell did in solving the problem of the solar system; but we would know, at least, just what it was we were trying to explain.

COMMENTS BY JOHN A. KEEL:

This is the kind of article that was so popular in the fanzines in the 1950's and early 1960's, but is now totally obsolete both in concept and content. It continues to amaze me that most of the American hardcore UFOlogists remain locked in the foolishness of the past, completely unaware of the enormous advances that have been made in the past decade. Since the hardcore is the most vociferous, armchair critics like Oberg, Cohen, Klass, etc. remain equally ignorant...and waste their time arguing with the ding-a-lings. By publishing trivia like this you are merely perpetuating the nonsense.

COMMENTS BY BRUCE MACCABEE:

After reviewing Greenwell's article and spending some time trying to decide what to say about it, I have concluded that basically the article is quite good, as far as reciting "theories" or "speculations" about the nature of the phenomenon (phenomena?) that give rise to UFO reports. (Here UFO is used in the "Hynekian" sense as a phenomenon which is described in a reasonably detailed report by a credible witness (es) and which remains unidentified after extensive investigation.)

My opinion is that we don't have enough solid evidence to make a selection between theories, although several of them certainly seem very remote (e.g., hollow earth, secret weapon, underwater civilization). However, I would tend toward the extraterrestrial theory because some of the UFO reports include descriptions of objects which appear to be machines capable of traversing our atmosphere. It is these reports which, if accurate, completely exclude the possibility that UFO reports result from some hitherto undiscovered but natural phenomenon. (Here "natural phenomenon" implies some real physical phenomenon which gives no evidence of intelligence.)

With regard to the "new natural phenomenon" hypothesis, I note that Greenwell has in effect bypassed this possibility by dividing unevaluated UFO reports into two possible classes: those exhibiting no indications of intelligence and those in which intelligent control is implied. He calls the "unintelligent" UFOs conventional and the intelligent UFOs unconventional. Of course, airplanes are conventional and "intelligent," so we have to understand that anything manmade which is described in a UFO report falls into his "unintelligent" category, along with the other "IFOs." A new natural unintelligent phenomenon would thus fall into the IFO category, when in fact it should be considered unconventional, even though unintelligent. In spite of my opinion as stated above that some UFO reports imply that intelligently operated machinery is traversing our atmosphere, I believe that the possibility that a new unintelligent phenomenon gives rise to some UFO reports cannot be ruled out at this time.

COMMENTS BY PAUL MCCARTHY:

This is a fine paper. Greenwell does an excellent job of presenting the eight unconventional hypotheses which purport to explain UFO sightings. His knowledge of the literature is impressive, and his concern for the historical aspects of each speculation is commendable. There are very few individuals in the field who could not benefit from the attention to detail and the little known facts which are related here. Moreover, the assessment of the likelihood of each hypothesis is concise and raises the basic objections which tend to polarize the respective debates.

However, while the above praise for the body of the paper is well deserved, what I would like to do in the remainder of my remarks is quibble with the first two paragraphs - the framework in which Greenwell develops his comments. The questions which arise involve terminology and the perspective which is brought to the work.

First I will address the question of terminology. In his initial paragraphs Greenwell draws distinctions which do not appear to be valid. He points out that we can divide explanations for UFO reports into two major categories - conventional and unconventional. But then he asserts that the former involves no purposeful intelligence while the latter does. Yet it is obvious that aircraft, hoaxes and possibly hallucinations do involve purposeful intelligence. This distinction, then, needs some re-thinking.

He then goes on to explain that conventional explanations are synonymous with IFOs. This is not always true. They are the same only in those cases and for those investigators who agree on the resolution of a given sighting report. For instance, Phil Klass, to the best of my knowledge, has never investigated a sighting that he could not put into the IFO category, although not everyone would accept Klass' explanations. This is because one faction of ufologists has adopted the distinction between IFOs (identified flying objects) and UFOs (unidentified flying objects), while the other has not. That is, if a sighting report is identified it becomes an IFO, but if it remains anomalous after its investigation it retains the UFO label. The latter, however, is only possible within the ufological paradigm which Greenwell represents. In the paradigm represented by Klass all investigations result in IFOs, or at least hypothetical IFOs, whereas in Greenwell's paradigm the outcomes are either IFO, hypothetical IFOs, or hypothetical UFOs.

This raises another point. While Klass claims that he can provide conventional explanations for all sighting reports, in fact he is placing some portion of his sighting investigations in the hypothetical IFO group. This means that while there are IFO data which all analysts would classify as such, there is a good deal which is in dispute (for Klass, all sightings which the opposing paradigm would classify as UFOs after investigation). It also means that Klass, as well as Menzel and others have hypotheses which bear on what some would consider "real" UFOs - the residue. Therefore, it would appear appropriate for Greenwell to give them their due, or in his title indicate that he is only interested in unconventional (are insects and ball lightning conventional?) hypotheses.

Lastly, this may indicate that we are reviewing a paper removed from its context, The Encyclopedia of UFOs, which would provide a rationale for the short-shrifting of "so called" conventional hypotheses. Or alternatively, it may mean that Greenwell has been hoisted on his own petard. For I believe he said within his ETH discussion that, "To deny the validity of a reasonable hypothesis because of an emotional commitment to other explanatory possibilities is not consistent with operational procedures in science, regardless of how learned such individuals may be, or how persuasive their arguments may appear."

COMMENTS BY AIME MICHEL:

In the Popperian sense,¹ theories and hypotheses are structures that (a) explain all that we know about something, and (b) predict things we do not know yet, in such a way that, if the prediction is wrong, it can be falsified.

It is obvious here that (a) we do not know exactly what we have to explain and (b) even less do we know how to elaborate falsifiable predictions. Then, it will be time to look for hypotheses about UFOs, when a scientific description of UFOs is available, which is not the case at all for now.

So I would propose an alternative way of reasoning.

According to those who have made the calculations,² when an intelligent species reaches a space technology "the entire Galaxy could be explored and all its habitable planets settled within the next million years" (Dole, p. 139). Considering the effects of the large number (of stars, planets, etc.), it is hardly believable that not a single intelligent species did appear in the past of our universe.

So the odds are for our Galaxy being occupied since time unknown by one or more species more advanced than ours. The astronomers have named "The Club" that thinking "milieu" predicted to live out there since times unknown and previous to man.

Then we are faced with the question: "Why is The Club invisible? Why does the prediction of astrophysics and biology, however rigorously inferred from trivial knowledge, seem to be failing?"

This is an unsolved scientific problem, a sort of cosmological paradox similar to Olber's paradox, though much more dramatic, since it challenges what we hope we know best, that is, the effect of large numbers.³

Perhaps one could propose a solution to that paradox as follows: "Actually, The Club is not invisible; actually it is part of our historical environment; only we have not yet been able to identify it as such."

Let us call that solution: "The UFO Hypothesis."

Notes:

¹ Popper, Karl R., Conjectures and Refutations. Routledge and Kegan Paul, London, 3rd edition, 1969, ch. 11.

² Dole, Stephen H., Habitable Planets for Man. Blaisdell Publishing Co., New York, 1964; and more recently the publication of P. O'Neill and co-authors about space colonization.

³ I have named that paradox The Grisonne Paradox after my cat's name, Grisonne. Why (my cat often asks me) are the heavens not full of cats? Why do cats not see everyday extraterrestrial cats landing by the millions on this Earth? Why, by the Great Cat, do they not get out of their flying saucers to help us to solve the main problems of this Earth, such as too many dogs, too scarce mice, and so on?

COMMENTS BY JAMES W. MOSELEY:

There is no way that I can use up my allotted 600 words in discussing Mr. Greenwell's article. I think that he has done an excellent job of presenting the various UFO theories objectively, concisely, and fairly. He has obviously had to do a good deal of painstaking research to write this short article, and he is to be commended for the objective way he has summarized the vast body of UFO literature he has had to wade through.

Proponents of one theory or another will write you, insisting that the author has short-changed the particular pet theory of the letterwriter, but this is to be expected.

I was intrigued by the author's remark to the effect that orthodox scientists would be more impressed with UFOs and more willing to study them seriously, if the sightings were far less frequent. Thus, ufologists may be defeating their own purpose by trying to present as great a volume of data as possible. This goes along with one of my own pet beliefs, that it is time we forget about presenting the public with hundreds of half-proven cases, and concentrate on just one totally proven case - if there is, has been, or ever will be such a case!

My thanks to you for including me in your list of "experts," and to Mr. Greenwell for writing such a thoughtful article.

COMMENTS BY JAMES E. OBERG:

To quibble over classifications, I must suggest that 'hollow earth' and 'underwater' are merely locales for basing, not any classification of nature/purpose of UFOs -- no different from tales of Mt. Shasta, Tibet, Antarctica, or similar remote (and uncheckable) regions. Such 'theories' belong under an expanded "secret weapon" class, modified to be "secret mechanical technology" with several sub-classes: A1-contemporary human / government; A2 - contemporary human / non-government; B - ancient but surviving human; C - relics of ancient no-longer-surviving human; D - native non-human. And I must point out that there are numerous published photographs which purport to show the holes in the poles. Further, one entire type of theory has been completely omitted: that UFOs represent some supernatural (or divine, or angelic and/or demonic) manifestations, a 'theory' that has great grass-roots appeal in fundamentalist Christian sects who are not particularly visible to mainstream UFOlogy.

For the ETH, I feel it is weak to belabor the difficulties of interstellar flight, and to dismiss the local Solar System origin of postulated UFOs; that increases the power, unfortunately, of the non-rational a priori fallacy that "they must exist out there, thus, what is more reasonable than that they are observing us". To go further out on a limb, for completeness I think it should be pointed out that there are several conceivable motivations for one or more ET agencies to refrain from overt

contact while allowing 'glimpses' and slow acclimatization. OK, so it's all just conceivable -- but I do not believe it is true, because my view of the evidence is that nothing of the sort is required to explain UFO reports.

Let's also discard the word "theory", with its implications of scientific standards. Such implications are totally unjustified for aimless speculations which have absorbed and 'explained away' any and all new developments in the body of 'UFO evidence' for three decades. These so-called 'theories' are non-disprovable, non-testable, and by this lack of true theory-hood have shown that "UFology" is not a science nor even a proto-science. At best, it can collect inputs and attempt to extract useful information, a function at which the vast majority of self-styled UFOlogists have been dismal failures.

Characteristic of these listed so-called 'theories' is that at best each of them only explains a subset of all UFO reports anyway: the reports they do not explain are discarded or ignored, and it can be a sizable fraction of the body of reports. The conventional explanation (that UFO reports are generated by mainly honest misperceptions and misrecollections of prosaic phenomena colored by people's imaginations and usually subconscious UFO knowledge, spiced with hoaxes, pranks, and hallucinations -- which collection by the way is bound to produce a sizable false residue of "unexplaineds" purely due to human perceptual/memory/investigative limitations --) does in fact explain a larger fraction of reports than any of the other 'theories', and is in my mind the only currently viable hypothesis to explain the origin of UFO reports.

As to how UFO reports are distorted, exaggerated, falsified, and selectively edited by the UFO media to give a far more convincing appearance than the original facts actually warrant -- aha, that question is for later discussion. But it explains why "most Americans believe in UFOs", for UFOlogists may denounce the media sensationalist exploitation of the subject, but they boast of the result. They, to use a Biblical metaphor, curse the tree but savor the fruit. And the 'science' of UFology remains unborn, and possibly even unconceived.

COMMENTS BY JOHN RIMMER:

J. Richard Greenwell's summary of several of **the** theories of UFO origins contains much that is of value. In particular, no further comment is required on his arguments for dismissing the Secret Weapon, Hollow Earth, Underwater Civilization, and Space Animal speculations. However, these beliefs are held only by a very tiny proportion of UFO investigators and theorists. Greenwell also summarizes neatly the principal objections to the Extraterrestrial Hypothesis--still probably the majority view amongst ufologists.

Some confusion appears to creep into Greenwell's arguments when he deals with the "Ultraterrestrail" and "Psychic Projection" speculations. Although there is a certain overlap between these two ideas, there are a number of very divergent viewpoints between researchers whom Green-

well would presumably consider as propounding one or another of these viewpoints. However, I think the confusion really begins in the first paragraph of the article, where Greenwell groups the hypotheses into two main categories: conventional, which he defines as involving no purposeful intelligence; and unconventional, which he considers does involve purposeful intelligence. This leaves no room for an unconventional explanation that does not depend on some conscious, intelligent manipulation or planning. He further confuses the issue by considering hoaxes and hallucinations in the "conventional" category. The former definitely involves a purposeful intelligence; and the latter definitely involves intelligence, though its purposefulness is perhaps the principal enigma of the UFO problem.

At this point Greenwell falls into the trap of producing by implication, his own definition of a UFO, which does not correspond to the observed reality of the phenomenon. The assumption of the rest of his article is that a "UFO" is a physically real object, or is at least (in his own understanding of the Psychic Projection hypothesis) a phenomenon which may manifest itself in a solid, physical form for a finite period of time, and is thus capable of effecting some more lasting change in the physical environment. Anything other than this is presumably an hallucination, and by Greenwell's implied definition has a "conventional" explanation and is thus not a UFO.

Greenwell seems to take the phrase "Psychic Projection" rather literally. He seems to imagine it rather like a movie projector using ESP to literally "project" a UFO image from the mind of one percipient to another. In fairness to Greenwell's critique, it must be said that this is the idea that some "Jungian ufologists" also appear to have gained of psychic projections. In fact, there is no need for this projection to be anything other than an internal projection from the unconscious to the conscious mind. Similarly, the "Collective Unconscious" need not be thought of as a sort of psychic CB Radio network, but simply as the collectivity of environmental, physiological and cultural impressions that form a background of shared experience to individual human thought and perception. It is this shared experience which the unconscious is able to draw upon to provide the symbolism which structures the psychic projections to the conscious mind-- Jung's Archetypes.

If this interpretation of "Jungian ufology" is valid, it would appear that the phenomenon is almost entirely internal to the individual percipient, although the forms of its perceptual manifestations may be broadly common to most experiences. However, this internal nature would seem to be enough for Greenwell to include such an explanation as a hallucination in his "conventional" category.

Greenwell is right to draw attention to the wooliness of many of the arguments used in "parapsychological" and "paranormal" explanations of the UFO problem, and rightly draws attention to the unsatisfactory nature of most UFO hypotheses, which are incapable of being disproved rationally. (It is this capacity, of course, which many ufologists mistake for actual proof!) However, like many of these ufologists, he begins his analysis with a too-limited definition already in his mind of what the UFO phenomenon is.

In summary, Greenwell makes good arguments against several speculations that have been offered, but seems confused over one, the "Psychic Projection" theory, that does seem to go part way to an explanation. He does not appear to consider explanations based on internalized, psychological factors as representing part of the UFO phenomenon, which he sees in mainly physical terms.

COMMENTS BY MICHAEL K. SCHUTZ:

In his paper, Greenwell has mentioned just about all the possible explanations for UFO sightings. Accordingly, somewhere among his nine hypotheses (counting "conventional phenomena" as one), there must lie the truth.

So, we have a multiple-choice test, then. Well, is it A? B? C? Perhaps- "all of the above"? How about "none of the above"? (No, not possible: the truth is in there somewhere.) But, when it comes to multiple-choice tests, my favorite answer is, "A and B, but not C." On final exams, that really separates the sheep from the goats. And that appears to be the situation here.

I would agree with Greenwell that many of these hypotheses would come under "C." They can be ruled out right off, given what we have learned in over thirty years of modern-day UFO investigations. To begin with, my candidates for "rule them out" would be unconventional explanations numbers (1): the secret weapons theory, (2): hollow-earth theory, (3): underwater-civilization theory, (4): space-animal theory, (6): time-travel theory, (7): ultra-terrestrial theory, and (8): the psychic-projection theory.

Most of these do not need much comment. Number (1) might have been plausible at one time, but no longer. Secrets do not keep that well or that long, for one thing. Numbers (6) and (7) may be attractive to some people, in a highly speculative sort of way, but they jump much too far beyond the facts at hand to be of any use, in my opinion. And the "psychic-projection" argument, Number (8), misses the real point. Like many UFO researchers, I have personally encountered cases wherein the witnesses dearly wanted their sighting to be "the real thing." (And that is a natural enough desire, to be sure.) But the point is not that they projected their unconscious needs and desires up into the heavens. Rather, they latched onto perfectly real, but perfectly conventional phenomena, and superimposed onto them their unconventional explanations. And that, I would say, eliminates (8) as a separate hypothesis.

That leaves two possible explanations: conventional phenomena and unconventional hypothesis (5): extraterrestrial visitors. These may well turn out to be the "A and B," of the "A and B, but not C" answer. It is common knowledge that 90-odd percent of UFO sightings have conventional explanations. In "Project Blue Book Special Report Number 14," which may still be the best piece of large-scale research ever done on UFOs (the Condon Report and other works notwithstanding), 2,199 sightings were analyzed. Only 19.7% were listed as "unknowns." And a majority of those, 175 out of 434 to be exact, were of doubtful or poor quality. But the striking thing is that 188 are listed as being of "good" quality, and 71 were listed as "excellent." And this is totally separate from the "insufficient information" category. So, 3.2% of the 2,199 were listed as excellent quality unknowns. That

alone should make one think that "A only" will not prove to be an adequate answer. We know that in the mining industry, for example, one has to go through thousands of pounds of ore, in order to find a few ounces of gold...

Cases come in, year after year, from every part of the globe, just as they should, if the phenomenon is really unconventional. Cases of high-strangeness and high-credibility, as Hynek would say, keep coming in, with locations ranging from Papua, New Guinea to Socorro, New Mexico. But that in itself presents a problem, says Greenwell. He states that the very "volume of traffic" serves paradoxically to undermine the extraterrestrial hypothesis. There are simply too many cases, for them all to be of extraterrestrial origin. That is a good point, but it can be answered. First of all, conventional-explanation cases and insufficient-information cases take up the great bulk of all cases, as we have seen. One is tempted, for example, to throw out almost all the notorious night-light cases, on the ground of inadequate information, and stick strictly to those cases involving full and clear visibility, in close-approach and/or broad-daylight situations. Secondly, many really-extraterrestrial cases would no doubt involve the same craft, seen in different places. The famous "Bayonne-Vichy" series of sightings in France, for example, would seem to be a case in point. Just consider these two points, and the "traffic" becomes a good deal more manageable.

And thirdly, we must give credit where it is due. Earth is an extraordinary planet, with all of its lifeforms, both intelligent and otherwise. Surely there are many other inhabited planets, even in this corner of the galaxy, but a planet like Earth has got to be an extraordinary rarity, and worth a lot of study. Does the volume of traffic in a city like Paris negate the idea of foreign visitors there? Of course not. Paris is an incredible city, and people from all over the world go to see it. And furthermore, getting to Europe nowadays is immensely easier than our ancestors could ever have imagined it would be. So, too, perhaps, with our Earth.

And it would be logical to expect that if we have visitors here from any other worlds, we probably have them from many other worlds. "Extraterrestrial Pluralism," one might call it. And this idea, extraterrestrial pluralism, does seem to be well supported by the evidence. People appear to see, in broad daylight, craft of different shapes, performing maneuvers of different kinds, some making brief landings, others just hovering overhead, and some, as in the Father Gill case, appearing to indicate indecision on the bridge, with gradual descent followed by rapid retreat.

So, the volume of real extraterrestrial traffic, if there really is any, is perhaps only a few percent of the total body of sightings, and there may be dozens of "different" sightings per craft. And if the traffic still looks large, there may be a good reason for it, given the nature of this planet, and given the travel capabilities of more advanced civilizations. And as

for the time and distances involved, remember Columbus, Magellan, and all the other great explorers: their journeys seemed almost impossible at the time, they took years out of men's lives, and they took fortunes to finance. But they happened, nevertheless. Perhaps there is truly a universal impulse to explore new worlds, to boldly go where no man has gone before.

Greenwell rightly states that many students of the UFO phenomenon seem to have tired of the extraterrestrial hypothesis. A pity. Just because the ultimate answers have not yet been revealed, that is no reason to abandon what is perhaps the most reasonable hypothesis around. Continued patience is required, as we continue to ask if there is truly an unconventional phenomenon here at all.

Reference:

Flying Saucers: An Analysis of Air Force Project Blue Book Special Report Number 14, edited and published by Dr. Leon Davidson. Library of Congress Catalog number 70-138935 (fourth edition, 1970). The data presented are on p. 24. On p. 94, the authors lament that "the data as a whole failed to reveal any marked patterns of trends." Right - it would be a good deal simpler if all "excellent quality unknowns" (their words) involved craft of exactly the same shape, performing exactly the same maneuvers. But the variety is indeed there, and that, too, is part of the mystery.

COMMENTS BY ROBERT SHEAFFER:

Mr. Greenwell has done a creditable job of summarizing the principal "unconventional" hypotheses about UFOs, and his list is reasonable comprehensive. I do not see reference to some of Vallee's more recent speculations, the "control system" hypothesis (in The Invisible College), or the "deliberate human deception" hypothesis (in Messengers of Deception), but of course there comes a point of diminishing returns where it makes little sense to dredge up increasingly obscure (and increasingly bizarre) hypotheses, just for the sake of completeness.

A few comments on what Greenwell has included:

The "hollow earth" theory goes back well before the early part of the present century. Proposals for expeditions to "polar holes" were made at least as far back as the mid-nineteenth century.

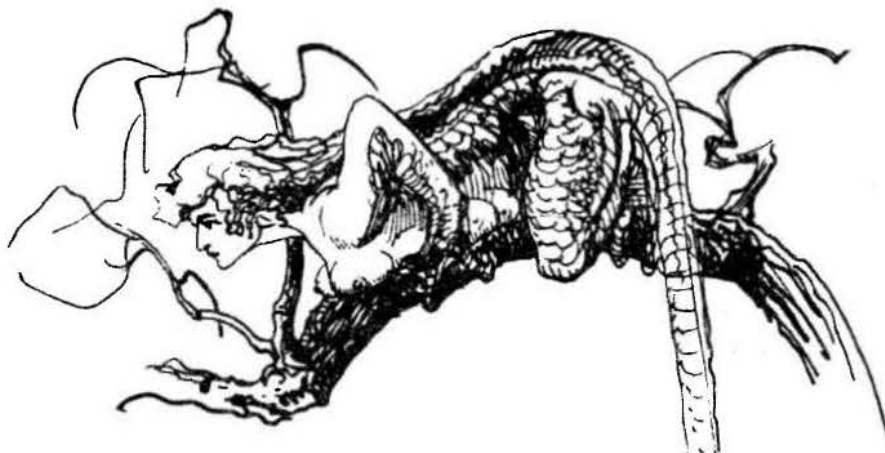
While it is of course true that earth satellites have not photographed any "polar holes", Ray Palmer and Brinsley Le Poer Trenchy (the Earl of Clancarty) have claimed that they have, publishing NASA photos which seem to show a large, gaping hole in the North Pole. However, they are simply misinterpreting a photographic effect of the months-long polar night around the time of the winter solstice.

The extraterrestrial hypothesis has been unfairly criticized by many scientists on the "volume of traffic" objection. But assuming that one accepts the ETH premise, it is entirely reasonable to argue that the UFOnuts have made only one interstellar trip (or at most a few), and have done a great deal of local travelling since their arrival. One UFO could make a lot of "short hops", from McMinnville, Oregon to Papua, New Guinea to Pascagoula, Mississippi to New Hampshire's White Mountains, etc., in very little time. Thus it is a fallacy to equate, as some have done, the number of alleged sightings of UFOs with the number of hypothetical interstellar voyages.

On the negative side for the ETH, however, many scientists are now challenging the once-universal assumption that life, and especially intelligent life, is positively abundant in the universe. ETH skeptics have been rapidly gaining ground in the scientific community, with astronomer Michael Hart perhaps their best-known spokesman. Hart not only maintains that the earth, in all probability, contains the only intelligent civilization in the galaxy, but may very well be its only planet with life of any kind. (See Hart, Quarterly Journal of the Royal Astronomical Society, Vol. 16 (1975) p. 128; New York Times, Nov. 4, 1979 p. 12).

A few comments on what Greenwell has left out: any detailed discussion of "conventional" explanations for UFOs. Unlike his treatment of the other hypotheses, no information is given to enable the reader to determine the adequacy of conventional explanations for UFOs.

The somewhat misleading impression is conveyed that the conventional hypothesis must "stretch" one of a limited number of prosaic explanations to try to cover every case, with any shortfall due not to possible "noise" in the testimony, but to inadequacies of conventional hypotheses. But a more accurate exposition would show all conventional explanations as being sub-classifications of the null hypothesis, the cornerstone of statistical analysis: until proven otherwise, it must be assumed that the sample group (reported UFOs) is just a subset of the control group (conventional objects). Thus the central task of UFO theorists is not to establish that UFOs are extraterrestrial or are time travellers, but whether or not there exists a UFO phenomenon which is distinct from all conventional phenomena. The entire thrust of the paper seems to me to be inverted: the reader is given a great deal of information that will assist in choosing among competing unconventional hypotheses, but virtually no information is presented to enable one to evaluate whether or not any unconventional hypothesis is warranted.



COMMENTS BY P.A. STURROCK:

The question I am going to address is: "From the Scientific point of view, does it make sense to formulate hypotheses concern-UFOs?" There are, I suspect, many scientists who would answer this question with a resounding "No," claiming that there is no point in formulating hypotheses until a solid data-base has been established. Such scientists might further argue that any new case should not be added to the data base if there is the slightest possibility that it might be due to a hoax or misperception of conventional causes.

I am going to argue that the opposite is true. It is my belief that we will make best use of the available data if hypotheses are formulated and if data and hypotheses are compared according to the normal procedures of science.

One can certainly imagine changes in the data base as a result of which my argument would become irrelevant. For instance, Condon pointed out that the UFO debate would immediately be ended (or at least transformed) if an alien spacecraft were to land on the lawn of a hotel at which a meeting of the American Physical Society were in progress, if the occupants were to emerge and present a paper explaining who they are, where they come from, and how their spacecraft works, and satisfactorily answer the searching questions of the assembled physicists. I concern myself with the eventuality (which seems to me more likely) that no such overwhelmingly dramatic, conclusive and public event will occur, but instead we will continue to learn of a sequence of cases--some poor and some rich in detail, some involving good witnesses and some involving poor witnesses. The question to be addressed is how one can best advance our understanding of the phenomenon in terms of a continuation of the present stream (some would say trickle) of admittedly soft evidence.

This situation is very different from that which is encountered in normal science. In our normal scientific activity, we have a very secure data base. This is not to say that, as a theoretical astrophysicist, I believe every result published by observational colleagues. When a group of observers announce a startling new discovery, I do not normally accept the announcement at face value. My practice is to wait and see if the discovery is confirmed by an independent group. Even after that has occurred, I wish to learn what information is available from other groups using other equipment, preferably recording data in different parts of the electromagnetic spectrum.

Yet even with these precautions, astrophysical evidence may not be as definite as one would wish. From the fact that two young pulsars are in supernova remnants, we tend to conclude that all pulsars begin with supernovae. This is a good inference, but it is not a conclusive deduction.

As I have pointed out elsewhere,¹ the very fact that we have only inconclusive data for many of the interesting problems of

astrophysics means that the science of astrophysics bears a closer relationship to the study of UFOs than does, for instance, most laboratory physics. Indeed, I have argued² that the most important stage of a science such as astrophysics is the stages at which unlikely hypotheses are becoming likely. Once they are established conclusively, the most interesting stage of the game is over. From this point of view one can say that, although the study of the UFO phenomenon is unlike the bulk of everyday science, it does resemble the most exciting part of the more exciting sciences.

The question, then, is how a scientist should proceed when he finds himself in a complex situation of uncertainty involving a new phenomenon and data which are incomplete and possibly insecure. My own answer to this question is that one must return to the fundamentals of science. My personal view is that the basis of all science is Bayes' theorem.³ This provides a procedure for updating one's assessment of a hypothesis in response to new evidence. This theorem may be written in the form

$$(H/EX) = \frac{(E/HX)}{(E/X)} (H/X) \quad (1)$$

in which (A/B) denotes the probability that statement A is true on the basis of the knowledge that statement B is true, etc. Then (H/X) is the probability of hypothesis H on the basis of initial knowledge denoted by X. E is the new experimental or observational data, and one wishes to evaluate (H/EX), the probability of the hypothesis on the basis of the initial knowledge X and the additional experimental knowledge E. Equation (1) shows that we may calculate the new probability in terms of the old probability if we can estimate the probability of the experimental result E occurring on the basis of the initial knowledge X alone, and in terms of the initial knowledge together with the hypothesis H.

In order to apply this procedure, it is essential that one have a logically complete and independent set of hypotheses. Hence the first question one needs to ask about the hypotheses listed by Mr. Greenwell is whether they are independent and whether they form a complete set. My conclusion is that, with minor modifications, one can rephrase the hypotheses in such a way that they meet this criterion. I hope to present this reworking of Mr. Greenwell's hypotheses at a later date.

A very important consequence of Bayes' theorem is that if a scientist sets his initial assessment of the probability of a hypothesis as zero, that value will never change, no matter what evidence is subsequently present to him. This means that he is so sure of himself that he is willing to close his eyes to any and all evidence which may come his way. Such certainty normally arises only in logical or mathematical contexts, not in real life. Certainly, in the study of such a nebulous phenomenon as that of UFOs, it is inconceivable that anyone should be so sure of himself that he would set the probability of any significant hypothesis as zero.

One can also show that he may not set the probability of any hypothesis as unity, because that also would imply that the probability would never change. Hence a scientist may represent his "prejudice" about any hypothesis by a probability ranging between but excluding zero and unity. Once this has been done, application of Bayes' theorem will show how his estimate should change in response to incoming evidence.

In the assessment of evidence, different investigators should be able to reach much closer agreement than they can in asserting their initial prejudices. In fact, the initial probability which a scientist assigns to a hypothesis is likely to be of no interest to anyone except himself. What his colleagues probably wish to know are the implications of any new evidence which may arise. This is given by the ratio $(E|HX)/(E|X)$ in equation (1). In principle, a scientist could answer that question by saying "The new case has increased my assessment of the probability of hypothesis H by a factor of 10 (or reduced it by a factor of 5, etc.)." If a "quasi-believer" and a "quasi-skeptic" are each honest in their assessment of the data, one would expect that they would be in substantial agreement in such an estimate.

Equation (1) deals only with the basic principle of scientific inference. In order to make the equation useful, it must be developed into a more complex form, for instance along the lines I have discussed elsewhere² in relation to the evaluation of astrophysical hypotheses. A basic requirement is that the assessment of hypotheses and the assessment of observational data should be conducted completely independently. This is best achieved if it is carried out by different people. Within the context of astrophysics, almost all observational papers are treated seriously, and so are most (but not all) theoretical papers. However, if in the same article an author presents his own data and his own theory to explain his data, this article will be viewed with great suspicion. In the same way, it is most desirable that activities involved in the study of the UFO phenomenon should become similarly well defined and non-overlapping. We need some people who (preferably working in teams) restrict their activities to case studies and summarize their findings as cautiously as possible. We need others who draw up catalogs of cases, parametrize the evidence and look for patterns. And we need others who propose hypotheses and analyze their consequences in such a way that the consequences can be compared with the pattern analysis.

In setting out a list of possible hypotheses concerning the UFO phenomenon, Mr. Greenwell has taken a very valuable step towards a longterm scientific analysis of this subject.

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COMMENTS BY DAVID W. SWIFT:

Richard Greenwell describes and evaluates eight theories about UFOs. His well-written article is a helpful introduction for newcomers to this complex subject, and people already familiar with the field may find it a stimulating review.

It led me to step back from close involvement with specific details to look at the UFO situation as a whole. The large number of theories about UFOs indicates the vitality of the topic, if not its reality. UFOs may or may not exist, but speculations about them certainly do. Yet it is sobering to realize that a third of a century has passed since UFOs burst upon popular awareness; but we still do not know what they are, nor do we even have incontestable clues. During this same period we have learned much about other things, from subatomic particles to the far reaches of the universe. We have unlocked the genetic code, walked on the moon, sent space craft past Saturn and devised schemes for communicating with civilizations thousands of light years away, yet UFOs remain as puzzling today as they were in the 1940s; perhaps more so, because we have tried the "obvious" explanations -- hoaxes, misidentifications, secret weapons, and even interplanetary space ships -- and these have not solved the mystery.

And so we have eight theories about UFOs. What other phenomenon is blessed -- or cursed -- with as many? Greenwell does not indicate which theory, if any, he favors; instead, he points to flaws in all of them. In this respect he reflects the present position of many serious thinkers about UFOs: there is no satisfactory explanation.

However, new theories continue to appear. For example, Greenwell's discussion of the Ultraterrestrial Theory mentions Jacques Vallee's ideas, as presented primarily in Passport to Magonia. Vallee's more recent work, particularly Messengers of Deception, published after Greenwell's article, presents additional thoughts which amount to a separate, ninth theory. For want of a better title, it could be called the "Superterrestrial" or "Deception" theory. Vallee suggests that a group of humans has found "a method for the projection of images controlled mentally...a way to project scenes at a distance, to control and amplify psychokinetic abilities of human subjects...to create entities." This group is manipulating the public's belief in extraterrestrial visitors to encourage irrational cults which will eventually undermine the rational structure of civilization.

Vallee further suggests that our understanding of this situation may be hampered by our traditional view of psychics, which is oriented toward space and time, whereas an associative model, exemplified by the data retrieval of a computer-based information system, would be more accurate and revealing. "Time and space may be convenient notions for plotting the progress of a locomotive, but they are completely useless for locating information"... "If we

live in the associative universe of the software scientist rather than the sequential universe of the spacetime physicist, then miracles are no longer irrational events."

Twenty or thirty years ago such speculations would have seemed absurd. Now I am more willing to consider them, since none of the other theories in Greenwell's Gallery of Grotesque Guesses satisfactorily explains UFOs either.

Perhaps the truth about UFOs may not even be suspected today, or perhaps it will combine parts of several present theories. After all this time we might have hoped that the number of serious contenders would have dwindled to two or three, but it has not. While some, like the Hollow Earth Theory, are no longer in the running, a number of others remain, due, to a considerable degree, to scientists' avoidance of the UFO problem. Until they look closely at it, we can expect continuing growth in Greenwell's gallery.

* J. Richard Greenwell's reply to all his commentators will appear
* in the next issue of ZETETIC SCHOLAR.

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*I would like to thank Virginia Baker, Vice President of The American Society of Dowsers, for the use of her library.

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 Health Research, Box 70, Mokelumne Hill, California 95245
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-- Thanks to Ivan W. Kelly.

REPLY TO THE COMMENTATORS ON "PATHOLOGICAL SCIENCE"

RAY HYMAN

When I first read the comments on my paper, "Pathological Science," I was both puzzled and astonished. The various commentators seemed to be reacting to propositions and arguments that I had not made. I even considered the possibility that Marcello Truzzi had mistakenly sent them someone else's paper.

After further consideration, I decided that the fault for what I considered to be incorrect readings of my paper may lie, at least in part, with myself. I tried to scrutinize my paper with as much objectivity as I could muster (even though at least some of the commentators seem to believe that such objectivity cannot be achieved and may be a capitalist myth). Sure enough, I found that I could read my paper in ways that at least partially justified some of the interpretations.

When I was preparing my paper, I was trying to accomplish something different from what I eventually chose to do. I changed my objective almost at the last moment prior to the delivery of the talk at the AAAS meetings. The actual paper still retains some of the matter that was relevant to the initial objective. The paper is awkward and ambiguous at various points because of this failure to completely eliminate traces of the original paper.

Originally I was going to examine what Langmuir and others have labeled "pathological science." Philosophers of science have so far failed to find any consistent criteria for distinguishing such rejected science from more acceptable science. The verifiability criterion, the falsifiability test, and various contemporary proposals to solve this "demarcation" problem all fail because they either include many obvious cases of bad science or exclude many accepted instances of good science. As a cognitive psychologist, I have tried to reconstruct the thought processes that underlie many of the "pathological" claims to compare them with those underlying the "healthy" claims. In most cases I cannot find any difference. And so I was going to argue that there was no "pathology," in fact, involved. The same sort of thought processes that lead some scientists to make claims that Langmuir calls "pathological" are just those that have lead the very same scientist to make claims, on other occasions, that have found acceptance within the scientific community.

But I dropped this initial plan for a number of reasons. I felt that it might be too complex to develop in a half-hour talk before a general audience of scientists. Also, I had already written about this matter in other forums. And a new idea occurred to me that I felt would be simpler to get across and would be relevant to the symposium in which my talk was scheduled (on Science and Pseudoscience).

Langmuir's definition of "pathological science" as "the science of things that are not so" is colorful but useless. Much acceptable science falls under this categorization. In addition, controversies

arise in science with respect to other sorts of cases such as missed discoveries, competing interpretations of things that are so, etc. Although Langmuir's definition is not helpful, his cases do stand out as deviant in another sense. They all involve attempts by the scientific community to reject them out of hand--to prevent by any means their entry into the regular channels for scientific evaluation and argumentation.

My first suggestion was that if there is anything "pathological" about such cases, the pathology was not to be found by looking into either the truth value of the claims or the manner in which they were justified. Rather the "pathology" was in the scientific community's reaction to such claims--a reaction that was entirely out of keeping with the scientists' own image of rational, fair, and dispassionate dealing with claims. My second suggestion was that such extreme reactions in trying to discredit the radical claims of otherwise credentialed scientists has a number of consequences for the conduct of more orthodox science. And these consequences, in turn, might foster the very "evils" that the guardians of scientific purity feel they are trying to expunge.

It is within this context that I will try to respond to the various comments. For convenience, I will organize my replies under a number of general headings. 1. What was I trying to accomplish?

At least seven of the commentators deal, in one way or another, with my intentions. I am depicted as urging upon my readers an open-mindedness and fair-handedness in dealing with deviant claims of fellow scientists. A few of the commentators see such a plea as an unrealistic ideal. Both Feyerabend and Hattiangadi point out that a scientist operates at any given time with limited resources. It would be self-defeating if scientists tried to give equal attention to every claim competing for their attention. Feyerabend censures contemporary scientists, not because they limit themselves to investigating only some of an indefinitely large set of possibilities, but rather because they pretend that they have actually dealt with all the reasonable ones.

I fully agree that it would be foolish for the scientific community to try to give "equal time" to all claims put before it. Probably the success of the scientific enterprise has come about from the enforced limitations within an area of inquiry to only certain kinds of problems handled within a narrow disciplinary matrix. In this way the ramifications of a certain paradigm become thoroughly exploited before ultimately being abandoned for a new paradigm.

Unlike many of the commentators, I did not consider my talk as a plea for open-mindedness in science. For one thing, I restricted myself to those seemingly radical claims made by scientists who had already established themselves in some orthodox branch of inquiry. I had nothing to say about the much more numerous claims made by individuals outside the scientific community. And for another thing,

I was not urging that all radical claims by accomplished scientists be given a hearing within the scientific forum.

What I did want to urge was that if the scientific community did take it upon itself to challenge such radical claims, that it do so according to those standards of fair-play and criticism that it employs in dealing with less radical claims. My concern was that the discrediting procedure for some radical claims was obviously carried out in ways that violated those canons of rationality and objectivity that the scientific community typically endorses. The consequences of such a non-rational discrediting process could be negative for both the deviant scientist as well as for the scientific community itself.

One sort of negative consequence that some of the commentators brought up was the possibility that the radical claim was actually justified and might contain important truth content. While obviously a possibility, my paper was not concerned with that consequence as such. I was more concerned to explore the cases of non-rational discrediting to see what lessons they might contain for scientists, historians of science, philosophers of science, sociologists of knowledge, and cognitive psychologists. One effect of the discrediting process makes it very difficult to extract lessons from such cases. The discrediting usually succeeds in banning from further consideration, at least in regular forums of education and scientific studies, the "discredited" claim. We learn about the successful "orthodox" achievements of Kepler, Newton, Wallace, Crookes, Richet, Lodge, Flammarion, James and other scientific greats who also put forth radical and paranormal claims. But we do not hear about their "failures" (as defined by the scientific community). Or, if we do hear about them, we do so only in passing and with insufficient detail to judge whether the deviant claims differed in cognitively important ways from the accepted ones.

It may be true, as Allen Debus points out, that the situation with respect to the rejected claims of scientists is changing. During the past 20 years, historians have taken interest in the mysticism of Kepler, the alchemy of Newton, the astrology of Bacon and the psychic interests of Wallace. These studies have been helpful in revising our ideas about how the scientist's various interests and beliefs interact with his orthodox scientific work and theorizing. But these pioneering investigations are still exceptions to the rule and have as yet had little general impact upon scientific and general education. And they are handicapped by the fact that the discrediting process kept these deviant interests of the scientists from being given a full airing during their lifetimes. Newton's alchemical notebooks, for example, were kept in code, and it takes great ingenuity and speculative hindsight to gain insights into what role they played in his mechanical and optical studies.

Because I do not view my purpose as advocating a general open-mindedness, I am not sure how to reply to critics such as Rockwell-- who welcomes my appeal to open-mindedness but disagrees with my reasons, or to Braude, Cooter, and Mauskopf who, each in his own way, accuse me of hypocrisy. Nor does it help matters when the commentators do not make it clear if they consider my "manipulative tactics" to be

the result of a conscious decision on my part or an unconscious bias that reveals itself. Presumably these critics diagnose my "true" intentions on the basis of my choice of words and examples, so I will postpone my reply until later when I deal with those matters.

2. My choice of terms and referents.

The majority of the commentators question my use of the term "pathological science"--both because of its connotations and because the distinction it implies does not exist. First, let me deal with the problem of reference. Is there in fact a distinction between two sorts of science such as that made by Langmuir? Here we get into the murky area of the so-called "demarcation" problem. I have no doubt that Langmuir's definition fails completely to separate two sorts of science. As far as I can tell, philosophers and historians of science have still not succeeded in finding a consistent way to discriminate pseudoscience from science, pathological science from science, or, indeed, any sort of systematic inquiry from science. It has been easy to generate obvious counterexamples to any criterion that has been suggested so far.

However, the fact that we cannot draw a clear boundary between "good" and "bad" science, does not mean that the distinction is either meaningless or useless. At the extremes, I think we can point to cases of relatively good science and bad science. And although conceptual, logical, and epistemological matters are involved, I feel that the matter is ultimately an empirical one. Can we find distinctive features that will enable us to classify consistently those cases we agree are good science as "good science"?

I handled this matter very clumsily in my paper. And many of the readers thought I was endorsing, or merely expanding upon, Langmuir's characterization of pathological science. What I intended to do, however, was to abandon Langmuir's approach and simply identify my cases by the six indicants that I listed as "the distinctive characteristics of these examples." These indicants were:

- 1) a scientist of acknowledged competence and accomplishments
- 2) surprises his colleagues by claiming the existence of a phenomenon or relationship that is considered to be bizarre or even impossible by currently accepted principles
- 3) the scientific establishment either ignores or attacks with hostility this bizarre claim
- 4) the deviant scientist, along with a few deviant supporters, sticks resolutely to his guns in the face of attacks and indifference
- 5) the bizarre claim is considered to be discredited in the eyes of the scientific community
- 6) the claim is banished from further consideration in scientific literature, textbooks and education.

I now realize that my attempt to isolate a category of cases for further consideration needs elaboration. I do not claim that such cases can be consistently isolated from regular scientific studies on the basis of truth content, justifiability, or logic. What I do claim is that such cases do exist. We cannot decide, at

least as of now, in advance that a particular claim put forth by a scientist will become one of these cases. This is because my indicants depend upon how the scientific community perceives and reacts to the claim. Some claims, even ones that are anomalous and controversial, are accepted as legitimate problems for debate and evaluation within the accepted scientific forum. Others are rejected out of hand. They are not allowed further consideration within the regular forum. It is not the claim as such that I labeled "pathological," but the manner in which the scientific community responds and disposes of it.

Even with this latter characterization, I admit that the term "pathological" is premature. We do not yet know if the nonrational discrediting process that I have described is simply a dramatic and more extreme form of the normal way that scientists actually operate or whether it represents a qualitative break with normal scientific practice.

Again I want to emphasize that it is the scientific community that distinguishes between two kinds of science. One kind is allowed to be conducted within the normal forum. The other kind, the one I called "pathological," is denied access to this forum without due process. This raises a number of interesting questions. What is it that scientists are responding to when they treat a claim by a colleague as "pathological"? The fact that neither scientists nor philosophers have succeeded in articulating consistent criteria for such a distinction does not mean that there are none. Presumably the scientific community is reacting to something. And one issue for future research is to see if we can isolate what that something may be.

My distinction has also been called into question because of the labels I employed. By employing "pathological science," I am accused of falling into "a 'sociology of error' which claims to 'explain' false science only by reference to orthodox science, and by that process (I reinforce) belief in the latter's supposed value-neutral veracity." Within Cooter's ideology, this neutrality of science is a myth created to justify capitalism. I also give away my "true" motives according to my critics by my use of terms such as "bizarre," "follies," and "failures."

Much of this sort of objection, I take it, is the sort of posturing that establishes one as a bona-fide neo-Marxist. Within certain contexts and among a certain domain of readers such attacks upon my latent motives and my witting (or unwitting) espousal of the capitalist party-line on the objectivity and rationality of science makes sense and arouses the appropriate sort of emotions. But for those readers, like myself, who are not thoroughly immersed in this ideological way of slicing up the world, it is more confusing than enlightening. Even if I had the ability and inclination to respond in kind to such attacks upon my latent motives and alleged ideological blinders, I do not think it would clarify matters to do so.

However, let me say this much about my use of such terms. They certainly are not value-free. In employing them I was thinking of how the scientific community perceives and characterizes the radical claims. In this context, the claims are "bizarre," "follies," and

"failures." Again, it is perhaps an awkwardness of my paper that I did not make it clear that the terms I was employing were chosen to reflect the scientific community's attitudes to such claims rather than the actual truth-value or justifiability.

3. My attitude towards anomalous and paranormal claims

Some of the commentators take me to task for prejudging the validity of the paranormal claims that I discuss. Rockwell finds me inconsistent when I admit that Wallace's and other such claims were never rationally examined and then jump to the "unexamined position" that the findings were "follies." Braude, Cooter, and Mauskopf make similar accusations. And this becomes part of the basis for accusing me of hypocrisy. I seem to be advocating impartial and fair evaluation of deviant claims. But, in reality, say these critics, I merely want a less emotional way of disposing of these embarrassments.

Even though Wallace's claim that a sunflower materialized by means of psychic force was never rationally examined by the scientific community, I did say that I did not believe his claim. I also labeled such claims as "failures." But I do not see any inconsistencies in this position. Braude, for example, remarks "that there are no grounds, as far as I can see, for condemning the studies of D.D. Home as 'failures,' or Wallace's investigation of psychic forces as 'ignominious failures.'" For example, Hyman's apparent assurance that the case is closed, so to speak, on Home flies in the face of the considered judgement of many competent people who have studied this material closely and thought about it...very carefully."

Although I did not refer to Home in my paper, I would have also been willing to cite Crookes' studies of Home's psychic powers as "failures." It is true that no one who has studied the reports of seances by Home or Crookes' accounts of his tests on this medium has come up with plausible ways he could have cheated or produced the alleged results by normal means. But the word "failure" is appropriate because these accounts did not lead to further research and systematic findings on such phenomena by later investigators. The scientific community refused to be convinced. And the subsequent psychic researchers have repeatedly expressed dismay that most of the feats attributed to Home have never been witnessed with other psychics. Claims such as those made for Home by Crookes and those made for other mediums by Wallace are what Flew properly puts into the category of singular and in the past tense. They have not been repeated; they have led to no lawful relationships; we do not know how much of what was reported depended on singularly unique circumstances that will never recur; etc. The burden of proof is not as Braude would have it on those who want to say it was not paranormal. The burden is upon those who do claim it was paranormal. In the sense that psychical researchers have yet to provide repeatable evidence that Home's type of exploits can actually occur, there is no reason, in my opinion, to believe that they did.

At any rate, as I have already indicated, the truth value of the rejected claims is not the point of my paper. Some claims are rejected out of hand by the scientific community. And this is what raises a

problem.

4. My ignoring of the social determination of scientific knowledge

Cooter is most explicit about my "failing to question the cultural specificity of modern scientific truth." One does not have to be a Marxist to realize the powerful role that social forces play in the determination of scientific practice and theory. I do not know an important historian or philosopher of science who does not acknowledge this fact. However, the exact nature of this role, for many of us, is still an open question, one that has to be settled empirically rather than by appeal to Marxist or other dogma. Nor is it clear that social determination precludes rationality and objectivity. This too is still open for many serious scholars.

Cooter raises the matter in a way that I feel is irrelevant to the point of my talk. Whether science can be rational, objective, or value-free in any sense, or in any degree, is currently a hotly-debated problem. But it is only tangentially related to the fact that scientists do in fact respond to some claims by colleagues by the discrediting process I characterized. The scientific community, in its own eyes, does believe that it deals with matters in a rational and value-free manner. This belief, as Cooter claims, may be badly misguided. But that does not change the fact that it is widely accepted. On some occasions, those I have labeled as "pathological science," the scientific community seems willing to abandon even the pretense of rationality and refuse some claims the right of a fair hearing. And it is this that raises a number of interesting questions for philosophy, history, sociology and psychology. Cooter writes as if the answers to such questions are already known and given to him by means of his neo-Marxist faith. Those of us who prefer to find the answers by empirical investigation are accused of "unscholarly dogmatism" and of peddling "junk propaganda."

5. The forced compartmentalization of Wallace's orthodox biology and his unorthodox psychical inquiries into separate worlds.

Cooter takes me to task for coming "to the mistaken conclusion that Wallace's mind was fragmented into orthodox and unorthodox scientific compartments." And Mauskopf accuses me of distorting the historical context of Wallace's commitments by implying "that Wallace was (or must have been?) a bifurcated individual with two unrelated interests, one scientific and the other pathological." I can only assume that both of these gentlemen were so caught up in the fun and games of showing how ignorant I was of both the history and sociology of science that they failed to appreciate fully what I was saying.

In the first place I said nothing about Wallace's mind being divided; nor did I indicate that he was a split personality. In the second place, what I did say about forced compartmentalization describes an objective fact that neither Cooter nor Mauskopf can deny. In the cases of Crookes, Wallace and other scientists whose claims were discredited in the manner I was describing, the outstanding feature was that they denied the regular scientific channels for arguing their claims. Wallace may very well have been of one mind and quite internally

consistent in his views which encompassed spiritualism, socialism, anti-vaccination attitudes, phrenology, and a limited version of natural selection.

But scientific cognition is not simply a function of an individual mind operating in isolation. And it is here that both Cooter and Mauskopf seem to be the ones who are seemingly denying the powerful role of socio-cultural factors in scientific knowing. What one knows and how one thinks about it, especially in science, is heavily influenced by the interchange with colleagues, students, rivals, and predecessors in the scientific marketplace. The paradigms, disciplinary matrices, and other contexts that Kuhn and others try to characterize are quite real and potent. Even the scientist who works alone in his laboratory is heavily influenced by his internalized versions of these disciplinary matrices. His formulating of hypotheses, his design of the research, his use of instruments, his choice of variables and measures, his manner of graphing, analyzing and summarizing the data, and his mode of presenting it are decisively colored by the anticipated and actual reactions of colleagues, referees, editors and the like.

When Wallace wrote articles on new plants and species, on natural selection, and on the geographical distribution of species, he did so within the context of the existing scientific forum. He got reactions from other scientific colleagues. They provided new data and arguments which amplified or challenged or supported his views. And he knew what he had to do to refine, change, or defend his ideas within the context of this arena. But Wallace was completely denied such a forum for his ideas on psychic matters. He was forced to discuss such ideas in an entirely different forum--one which had different standards of proof, was already tolerant of such ideas, was less standardized in both the presentation and justification of claims, was of quite a different makeup and much less homogeneous than the scientific forum. Inevitably, the feedback, critiques, and types of responses he had to make in this second forum were quite different in stringency, shared assumptions, and other disciplinary features than the scientific forum within which he explicated his less radical ideas.

Everything that I know about cognitive psychology indicates that the ideas developed within these two very different forums, despite the fact that they originated within the same mind, would become organized separately in memory and would behave quite differently. And, yes, in an important sense, it could very well result in a form of dissociation or split within the mind of the individual who harbors both sets of beliefs.

To me this possibility may contain important clues about how to tackle some of the problems about rationality, objectivity, and commitment in science. I would have liked to have seen Cooter's and Mauskopf's thoughts on this matter if they had fully considered its implications.

I have not tried to answer each specific comment, but I appreciate the time and thought that each commentator devoted to the task. They found much more in what I originally thought was a simple talk than I imagined possible. Even when the comments were caustic and unflattering, they stimulated interesting thoughts and forced me to reconsider what I was trying to accomplish. Many of the commentators were obviously using my remarks as an excuse to ride their own hobby-horses. But there is nothing wrong in that. I think most of the commentators misread both my intent and my message; but, as I indicated, I believe I am at least partially the blame for these varied readings. I originally thought I could get away with making a simple point in an uncomplicated way. But if my talk revealed naivete on my part, it was with respect to this idea that I could isolate and talk about a simple idea in this very complicated matter about what constitutes good and bad science.

ZS Dialogues

JOSEPH MAY REPLIES TO GEOFFREY DEAN (ZS, #6):

I thank Geoffrey Dean for calling my attention to J.E. Wood's Sun, Moon and Standing Stones, which, while written from a uniformitarian perspective as is most astronomical literature, still furnishes a useful compendium on the subject of the megalithic standing stones. Because of my busy schedule, I have not been able to read the book completely, but I have read it in part and did manage to dip into it thoroughly. Unfortunately, I am unable to find anywhere the figure "0.05" degrees which Dean quotes. That is not to say that the figure does not appear in the book--it probably does--but Dean neglected to list the page number for his claim. My premier question is, on what page does this figure appear? I cannot evaluate a particular claim until I can check the context in which it is made. Exactly what am I called upon to explain? Please give a better definition of the problem and furnish the page, as well as the book, in which the evidence to buttress your argument is presented.

I think Dean's letter presents the opportunity to make two more points. First, I am genuinely puzzled by his phrase, "targets set by an undisturbed orbital geometry." Whose system of geometry is he referring to? Gerald S. Hawkins? I know that more than one attempt to decode the stones has been made. To which system is Dean assigning infallibility?

Finally, although Dean's letter is polite, and this is appreciated, it is illustrative of a tendency on the part of many of Velikovsky's critics, or perhaps I should say on the part of many who have made a brush and a scrape with his thinking, to conclude in exasperation that this or that item is conclusive evidence against the system. I think that nearly everyone who "gets into" Velikovsky has this kind of reaction somewhere along the line. Interestingly, the point where each individual bolts the train is different. Where this kind of a reaction is a problem, my advice is, hold up, don't jump to conclusions; place the issue under probing and even-handed scrutiny. After a thorough examination, then and only then, decide which side has the better case, Velikovsky or conventional science. It is important not to prejudge the outcome of that query.

LAURENT BEAUREGARD COMMENTS ON RAY HYMAN RE "PATHOLOGICAL SCIENCE" (ZS #6):

Hyman's thesis is that the scientific community should not react pathologically (irrationally) to a piece of "pathological science." Samples of pathological science are Wallace's and Crookes' experiments with, and theories about, "psychic force." Examples of pathological reactions to such experiments and theories include

shocked disbelief, embarrassment, attempts to ignore it, open hostility, ad hominem attacks on character, refusals to listen to arguments or view evidence, misrepresentations of claims, censorship . . .

and so on. All of these reactions are classed as attempts to discredit paranormal research, and we are reminded that to "discredit" deviant hypotheses by deviant means is very different from disproving the hypotheses, even though, by some lucky accident, discredited hypotheses usually turn out to have "deserved their fate." QUESTION: How can anybody know that discredited paranormal hypotheses have deserved their fate if the scientific community has never been rational enough to deal with paranormal claims in a truly scientific manner?

If Hyman's own thesis in this paper is to be supported, then we need a fairly clear idea of what it means for a paranormal claim to be taken seriously. Can Hyman make the judgment that yesterday's discredited paranormal hypotheses have deserved their fate? ("Deserved" here in a logical/empirical sense.) Surely a believer (like D. Scott Rogo) would take this as question-begging, since Rogo for one would claim that the most famous psychic cases of yesteryear (some--but not all--of the Palladino performances for example) have never been shown not to have been genuine paranormal manifestations. Some people today still swear by the Feilding-Baggally-Carrington series of investigations: they would say that apart from possible trickery here and there, there remains a hard core of genuinely unexplainable material in the Palladino case.

Hyman holds that the scientific community should not "banish the failures" by "inept discrediting procedures." I think here that we need to distinguish several kinds of scientific "failures" and that once we do this, it will turn out that some of these failures will not have been worth the tumble to begin with, while others will have been failures from which one could have--and still can--"learn(ed) a lot". The "great failures" may turn out to be very different (in kind, in structure) from "the perennial deviances." (Unified field theory vs. psychical research: not all failures are equally impressive.)

Hyman must be presupposing some such classification, since he holds that "most claims of pathological science" do not "deserve

serious consideration in themselves." Hyman says that the claims of Wallace (or of Crookes) about psychic force have no chance (or very little chance) of being true. QUESTION: But what logical justification is there for saying this? Have the claims of Wallace and Crookes ever received the fair objective impartial hearing that they "deserve"? (Deserve--in the sense of "consider the source"?) If they have, then Hyman's thesis is not needed. If they have not, then Hyman is not justified in saying that the claims do NOT "deserve serious consideration in themselves."

Hyman's whole argument seems to be urging that the scientific community NOT IGNORE paranormal claims. In other words, that time and energy and money be spent in "rationally" pursuing the claims (in the hope of refuting them, let us note). The alternative, of course, is to ignore the paranormal. And this is surely very different from the kinds of VIRULENT CRUSADING which Hyman describes and (rightly) argues against.

Hyman has not shown that it is irrational or inappropriate for scientists to ignore the paranormal, the perennially "coming science" (Carrington, 1908) which seems never to stop "coming" (Targ & Puthoff, 1976). Many scientists have made the considered judgment that psychic matters, UFOs, and so on are not worth the time and energy that they would require. Many scientists consider that there are priorities of scientific interests and that paranormal considerations are just pretty low on the list. I do not see any obvious irrationality in taking such a stand. It was essentially Condon's last stand on the UFO matter, and such stands retain the status of rational testimonials despite any all-too-human background of heated controversy surrounding the subject matter at hand.

It may be that the general public has much to learn by engaging in cool and rational close encounters with anomalies of the paranormal kind. It may be that science should refrain from polemics, histrionics, emotion, and steam in its off-hours crusades. But it does not follow at all that science should devote any attention to paranormal claims.

RAY HYMAN REPLIES TO LAURENT BEAUREGARD:

Some of the other commentators raise the same questions that Beauregard asks. "How can anyone know that discredited paranormal hypotheses have deserved their fate if the scientific community has never been rational enough to deal with (them) in a truly scientific manner?" I find this a difficult question because it opens up a number of entangled problems.

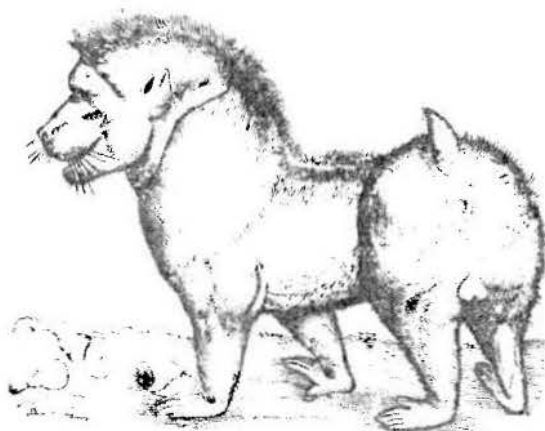
For one of my objectives the answer, as such, is not relevant. The issue is not whether they "deserved" their fate - i.e., ending up in oblivion. Rather, the issue is what is it that causes the scientific community to engage in emotional, non-rational discrediting procedures. Can we find consistent and distinctive features of certain kinds of anomalous claims that provoke such reaction? It

could very well be that we might find such features and that such features, when fully articulated, actually justify discrediting the deviant claim. On the other hand, it could be that the features that elicit the discrediting response have little to do with either the truth content or the arguments in favor of the claim. They might, as Hattiangadi hypothesizes, merely be signs that the controversial claim just is irrelevant for dealing with the problems that are seen as currently important.

Regardless of whether the offending claim deserved, in some sense, its ultimate fate, are there negative consequences for the conduct of science in the manner in which the claims are discredited? I was suggesting that there very well might be.

I also wanted to point out a possible advantage to the scientific community and others such as educators, historians and philosophers, of taking seriously the anomalous claims of otherwise accredited scientists. As several commentators pointed out, it would be unrealistic to suggest that scientists consider every anomalous claim put to them. But it does seem reasonable to take such claims seriously when they come from scientists of acknowledged accomplishments such as Crookes, Lodge, Wallace, Richet, Pauling and the like. If their anomalous claims do fall short of some scientific standards, it would be worthwhile to pinpoint the ways in which they were deficient. This would in turn help us diagnose just what went wrong and perhaps develop ideas to prevent such mistakes in the future.

If, on the other hand, the anomalous claims cannot be discriminated by any consistent criteria from the more orthodox and accepted claims put forth by the same scientists, then this, too, can be of great value. It would warn us about limitations of scientific cognition and it would reinforce the current notion that there are nothing but irrational or non-rational reasons for considering some problems as legitimate and others as illegitimate science. At any rate, I think we can learn much by trying to understand what is occurring both in the minds of the deviant scientist and in the reactions of the scientific community in these controversial cases.



BRADLEY DOWDEN COMMENTS ON ROBERT G. JAHN'S "PSYCHIC RESEARCH: NEW DIMENSION OR OLD DELUSIONS?" (ZS #6)

I'm uncomfortable with the advice Robert Jahn offered to researchers in parapsychology in the last issue of ZS. Jahn does recognize the case has not been made that there are any psychic phenomena which could not be explained in terms of known science. However, in his paper he underestimates the importance of directing future researchers to making this case and instead overestimates the importance of research into speculative theories that conflict with some of the most highly justified general principles of contemporary science. He also overestimates how much attention the field of parapsychology deserves from contemporary scientists.

I suspect he would defend his advice for increased theoretical research by asking how one could ever make the case for the reality of psychic phenomena without supplying an accompanying explanation of those phenomena, which in turn would require accepting hypotheses incompatible with current science. To this defense I'd like to make two comments.

First, he fails to emphasize that the theory-building efforts should be very hesitant to conflict with the basic limiting principles of contemporary science. These basic principles include the belief that no effect can occur before its cause, that one's mind cannot directly affect the external world except via an accompanying body, that the external world cannot directly affect the mind except via a sense organ, and that there can be no mind without an accompanying body. Science is replete with general principles, and it is not easy to say which are more basic than which, but these examples are usually accepted as being among the least apt to be revised when reform is imminent. Better advice from Jahn would be for caution--that if any new theories are called for, minor reforms should always be tried unless the grotesque complexity introduced by all foreseeable minor reforms indicates the necessity of major reform. There are always many revolutionary theories which will be compatible with the optimistic interpretation of the paranormal "evidence." For instance, there are always theoretical constructs like angels and demons to be conjured by the optimists as the cause of paranormal phenomena. More sophisticated optimists would appeal to more sophisticated theories like those four outlined on pages 13 and 14 of Jahn's paper. But what they have in common is the methodological error of underemphasizing the significance of the world-view compatibility criterion in choosing among competing explanations of the phenomena.

My second comment in answer to the question "How could the case for psychic phenomena ever be made except via an accompanying revolutionary explanation?" is that too much attention is being focused on making the positive case. The point should be to make or break the case. And the only appropriate way to do this is by trying to falsify the case. It is only when repeated, broadly-conceived attempts at falsification have failed that the positive

case gains inductive justification. It's only when we're pretty sure that there is something wrong with the null hypothesis which says the seeming paranormal results are spurious, that more theory-building of Jahn's sort would be called for. Being so optimistic about the need to create theories which make the positive case by overthrowing well-accepted principles is an unscientific attitude. Does Jahn have a good reason why there must be a new science waiting to be uncovered by future research? His advice fails to say it is more probable that "there is no there there," as Gertrude Stein might have put it. To his credit, Jahn does list some experiments which he claims "provide sufficiently provocative anecdotal evidence to justify further serious and systematic study." Even if these were so (and I'll let other commentators deal with that point), it is premature to be optimistic. The unlikelihood of the reality of the evidence points to the unlikelihood of reproducing the anecdotal results and to the likelihood that there is some overlooked non-random sampling error in the experimentation. Jahn would have been giving better guidance in his paper if he had suggested that nearly all the parapsychologists' attention should be devoted to the reproducibility question and to the search for sampling bias, not to speculative theorizing.

I'd like now to turn from Jahn's defense of increased theoretical speculation to his defense of increased activity per se in the field of parapsychology. Like his previous defense, this, too, is not well-organized and explicit, but is embedded in several scattered comments. To overcome current science's reluctance to promote parapsychology Jahn apologizes for the slim deviation from statistical insignificance that is found in most parapsychology experiments by remarking that "numerous areas of modern science percolate contentedly on lower statistical yield than is offered by some of the better psychic studies." This remark demonstrates a failure to appreciate the importance of the world-view compatibility criterion, if I may make this point again. Those other sciences do not produce data whose theoretical account seems to require the rejection of any of the basic limiting principles of science. Since parapsychology does claim to produce this data, and since so many of its past experiments have been explained by modern science, it is quite right that it be required to make a stronger case that its data is indeed non-spurious before it will receive the same encouragement to "percolate."

In another defense of his advice for increased activity Jahn says "everyone should be entitled to his own informal and considered opinions on such questions, and should be equally entitled to the tolerance of others toward those opinions." He also comments that the really significant question is "Do we have the right to inquire?" These comments are misdirected. Freedom of inquiry is all well and good, but the significant question here is not about the right to inquire, but "What are the promising lines of inquiry?" This is really the question he is answering when he says his "unconventional possibilities may be worthy of more detailed examination."

It would be unwise to further defend Jahn's advice by saying "The establishment laughed at Galileo." The analogy is worth very

little. Of course great leaps in knowledge come from considering daring new viewpoints. But open-mindedness is not the lone criterion involved. After all, the most open-minded person is the one whose mind changes with every good or bad argument before it. What future researchers need is the "nose" for retaining what's right about past knowledge while being open to rejecting the few bad apples. They need the "nose" for the promising new viewpoint. The person offering guidance to future researchers can only harm creativity by directly suppressing what he (or she) recognized to be unprofitable new viewpoints, but it is his duty to steer his creative student away from them. Jahn might call this steering "indirect suppression," emphasizing the suppression aspect. But that would be unfair. Political revolutionaries in America complain of suppression because they don't have equal influence on the information media to that of the establishment, but that sort of indirect suppression is rightly called "political freedom." Parapsychology quite rightly deserves its present low status within the field of psychology.

ROBERT G. JAHN REPLIES TO BRADLEY DOWDEN:

I appreciate the opportunity to respond to Mr. Dowden's comments, but since he seems largely to be contending with himself, or at least with his own inaccurate presumptions of my opinions on various aspects he raises, I am somewhat loath to intervene in his debate.

On one point, however, we may have a legitimate quarrel. His paternal concern for the potentially wasted efforts of his students and colleagues notwithstanding, the "significant question" remains precisely that of freedom of inquiry. Of course the burden of proof must lie with the advocate; of course this proof must be especially solid in an area such as this; of course one should assess realistically the validity of past efforts and the chance of success in the future; of course one needs a good "nose" for research; and of course there is an obligation to share our experience in such matters with our students and peers. But having affirmed all of this, to lay on a teacher the "duty to steer his creative student away from (unprofitable new viewpoints)," is to proclaim a hierarchy of insight that is inherently degenerative. I have learned far too much from my students to accept such responsibility on their behalf; to impose such judgment on my professional peers would be yet more presumptive. Authoritarianism such as this encourages established knowledge to sit smugly on its duff and categorically reject all new evidence that does not support or fill in its contemporary "world-view compatibility criterion"--whatever that is. Worst of all, it stifles the most precious attribute of human consciousness, the yearning for ever new, ever higher wisdom that has driven the mind and spirit of man to evolve upward, rather than merely to replicate.

If we must prostitute Gertrude Stein to embellish our point, the essential question might rather be put thus:

"Whose nose knows?"

Or perhaps better in the unabridged version:

"Who knows whose nose knows?"

DAVID MORRISON REPLIES TO JOSEPH MAY AND LEROY ELLENBERGER (ZS #5) AND TO C.J. RANSOM (ZS #6) REGARDING VELIKOVSKY'S THEORIES:

The Velikovsky debate generates a large measure of passion and an accompanying dose of personal abuse. Having been worked over in issues 5 and 6, I feel a response is required to clarify some of the fundamental problems of the pro-Velikovsky apologists.

In issue #5, Joseph May classes me among the "dogmatic" critics of Velikovsky, suggesting that I view Velikovsky as "automatically" in the wrong. This is a foolish criticism, implying that scholarship requires a suspension of judgment even when the evidence overwhelmingly argues against a hypothesis such as Velikovsky's. After nearly a decade of participation in this debate, it would be hypocritical for me to pretend that I could not discern the ever growing inconsistencies between Velikovsky and the factual evidence of astronomy and geology. But I can also appreciate the confusion of May and others like him who have not understood the procedures or the results of the physical sciences. May's confusion between "less than" and "equal to" (in his third paragraph) betrays how out of his depth he is. Does he not understand, in the example of timescales for Io, that a surface "less than a million years old" is perfectly consistent with the volcanic alterations on Io which we now know can take place on timescales as small as a few years? One or a hundred or a thousand or 999,999 are in full agreement with an upper limit such as "less than a million." No wonder the Velikovsky debate is confused, when such fallacious reasoning is used to conclude that "likewise, it appears likely [sic] that the ages of other bodies in the solar system have been incorrectly derived."

I would draw a different lesson from the Voyager discoveries about Io. The fact is that scientists were quick to revise their preconceptions and accept the idea of an object of unprecedented geologic activity, when presented with evidence to this effect. In the same way, they would have accepted evidence for a recently melted lunar surface if the Apollo missions had so indicated. But such evidence has not been found. Unfortunately for the defenders of Velikovsky, the flood of data on the Earth and planets acquired during the past two decades has consistently contradicted the basic hypotheses of Worlds in Collision.

May also objects to my criticism that he cites only pro-Velikovsky literature. The point is not that he should ignore the pro-Velikovsky literature, but that he should have verified its accuracy in factual matters by comparison with primary sources. He did not, and therefore his article repeats the frequently misleading and sometimes false claims that so often appear in PENSEE and KRONOS. Had he so checked, I doubt he would have asserted that a prima facie case existed for Velikovsky.

May's arguments for open communication and debate of unorthodox hypotheses are well taken. But in Velikovsky he picks a bad example. By now, this particular issue has largely resolved itself. Velikovsky has had a reasonably thorough exposure, and he has been

found wanting.

Ellenberger, writing in the same issue, questions my ethics as well as my logic. His personal attacks are contemptible, but his substantive arguments deserve discussion and, if appropriate, refutation.

Unfortunately, there is almost nothing of substance in Ellenberger's letter. Presumably he did not have the time to address specifics. He refers to Talbot's "correct" calculation of the cooling of Venus as if it were the final word on the subject, when in fact the calculation is even more naive than my own oft-criticized order-of-magnitude analysis; it would be laughed out of court by those who calculate models for the thermal evolution of planetary bodies. He also makes the claim that electric discharges can "greatly accelerate radio isotope decay, thereby resetting atomic clocks." Poppycock! Ellenberger, quote the evidence for this fantastic claim!

In issue #6, C.J. Ransom expresses surprise at my comments and asserts that the Velikovsky supporters read every detail written by his opponents. Alas, they do not read the same scientific literature that I see in astronomy or geology, or if they do, they do not understand it.

Ransom implies that I and other members of the scientific community are unaware of S.K. Vsekhsvyatskii and his lifelong work on cometary orbits, including the proposal that short-period comets are ejected from Jupiter or the Galilean satellites. I can assure him and your readers that I do know of Vsekhsvyatskii's theory, but I also know in what low repute it is held by the great majority of scientists working in the field of dynamical astronomy. The quantitative arguments which have to do with the statistics of short-period comet orbits are complex, but I do note that no plausible physical mechanism for such an origin has been suggested, and that no comet has ever been seen to originate in the Jupiter system. I also know, and it must be obvious to the readers of ZETETIC SCHOLAR, that these points are not relevant to Velikovsky, who proposed an origin for Venus (which is a billion times larger than any short-period comet) in a major disruption involving the two largest planets, Jupiter and Saturn. It seems to me that Ransom's comments are a good example of the misuse by Velikovsky supporters of the term "comet"; they argue from historical evidence for the application of this descriptive term to Venus, and then they try to draw physical inferences from comparison with contemporary comets, which are quite a different matter.

Ransom also asserts that the current view of the solar system has imperfections and unanswered questions, and that scientists frequently make mistakes. Of course. But this does not permit one to discount the often fundamental inconsistencies between Velikovsky's ideas and the astronomical evidence. Let me mention briefly one crucial example--the Moon. In his writings Velikovsky has made a number of predictions on the effects on the Moon of the violent near-encounters with Venus and Mars that he believed took place within

historic times. These included: extensive heating and surface melting, "the Moon's surface was repeatedly molten and its surface bubbled"; intense radioactivity "far exceeding any exposure regarded as safe"; strong seismic activity, "moonquakes...so numerous that [the Apollo 11 astronauts] may experience a quake"; surface rocks rich in such volatiles as oxygen, chlorine, sulfur, and water; and surface deposits of "hydrocarbons in the form of dried naphtha, bituminous rocks, asphalt, or waxes." In contrast, we now know that the Moon is a geologically dead, desiccated body. Specifically, the last major melting was associated with the Mare lava flows more than three billion years ago; the radioactivity level is very low, at the level expected for basaltic rocks; the seismic activity is more than a factor of ten lower than on Earth; the surface is strongly depleted in oxygen, chlorine, and sulfur, and is almost totally devoid of water in any form; and there is an absence of any organic material on the surface except at the very low level maintained by infalling meteors. For a further discussion of these and many other interesting points, I refer the interested reader to an article in the July 1980 ASTRONOMY by J.E. Oberg entitled "Predictions in Collision."

In the face of such evidence, Velikovsky's theories are simply not tenable. It is for these reasons that most scientists ignore him. Any new idea supported by some evidence deserves open-minded consideration, but if additional evidence fails to support it, as is most frequently the case, we usually put it aside and devote ourselves to more fruitful areas of investigation. After thirty years, Velikovsky's theory has lost its novelty, and the evidence against it is overwhelming.



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SOLOMON E. FELDMAN COMMENTS ON EDWARD W. KARNES, ET AL., RE
REMOTE VIEWING (ZS #6):

Although I am very much on the skeptic side of the belief-disbelief continuum, it seems that Karnes, et al., missed Tart's point in his critique of their experimental investigation of the "Targ-Putoff effect." While the outcome certainly puts another nail into remote viewing's credibility, the study itself suffered from the same shortcomings of many Pro-Esp studies. I refer less to the possibility of inadvertent Experimenter cueing per se, than to the fact that the experimental dice were loaded in one direction. In this study the only two possible outcomes were accept (i.e., fail to reject the null) the experimenter's hypotheses or "uncertainty" because of the many possibilities of experimenter cueing, cheating, etc. It would be wise to take what steps that we can to rule out obvious alternative hypotheses on both sides of the fence so that the outcome would more clearly support acceptance or rejection of the experimenter's hypothesis.

PIET HEIN HOEBENS COMMENTS ON EDWARD W. KARNES, ET AL., RE
REMOTE VIEWING (ZS #6):

In Zetetic Scholar Number 6, Karnes, Susman, Klusman and Turcotte report a failed attempt to replicate Targ and Puthoff's controversial remote viewing experiments. I would hardly have been surprised if they had been successful as their reported procedure seems to leave a loophole that could be used by unscrupulous psychics to manipulate the judging.

They allowed their subjects to self-select themselves into sender-receiver pairs. Moreover they asked the judges to take into account not only the actual target sites, but the description as provided by the sender and even his subjective experiences during the sending situation as well.

Both conditions together would enable sender and receiver to arrange a code before the actual experiment starts. Using the code would almost certainly result in a few spurious extra hits. The sender is allowed considerable freedom to select the elements of the target site he wants to register. He may concentrate on certain details while ignoring others. He may interpret the sending environment as he likes and even go as far as to remark on details that aren't there at all (subjective impressions). It is he who decides from what angle to use the motion camera. Given such freedom to select, almost any site can be made to fit almost any set of psychic impressions. The crucial point is that a sender and a receiver can agree beforehand as to the sort of elements the former is going to select.

A simple example: one of the agreed-upon elements might be "circle". During the experiment, the receiver will pretend to receive all sorts of circular impressions. Meanwhile, the sender searches his target site for circular elements. (traffic signs, tires,

clocks, flying saucers etc.) and concentrate on these. Whatever the site may actually look like, the judge knows what set of sender's material belongs to what site. He will be invited to look at the site with the sender's eyes and will, moreover, not fail to notice the striking similarities between the sender's report and the receiver's impressions.

As an additional form of manipulation, sender and receiver could use a third confederate, unconnected with the experiment. The role of this confederate would be to follow the sender and his escort to the target site and there create a pre-arranged scene that will be duly filmed by the sender. Simple examples would be: falling off a bicycle, bringing a huge dog, pretending to be drunk etc.) A variation would be for the sender to e.g. "accidentally" bump his head on some object found at the target site, the receiver at the same moment complaining about a sudden headache. As the judge will learn of both the "accident" and the "head-ache" he will have an additional reason to match the correct pair.

On rare occasions the code will not work (it might be difficult to get a huge dog in a church if that happened to be the target site), but then perfect scores are not required. A few extra hits will soon result in significant deviations from chance.

Of course, this plot would require collusion between subjects. I do not wish to imply that members of the Whole Life Learning Center in Denver, Colorado, are given to such pranks. On the other hand I feel an incorrigible hoaxer would find the idea to infiltrate such a lofty group irresistible.

EVAN HARRIS WALKER COMMENTS ON EDWARD W. KARNES, ET AL., RE REMOTE VIEWING (ZS #6):

I have just read Karnes et al.'s (1980) "Failure to Replicate Remote-Viewing Using Psychic Subjects" with considerable interest. I was struck by the large number of departures from acceptable experimental design as would be adequate to establish relevance to the question of the replicability of the remote viewing phenomenon (i.e. as exhibiting the existence of some psi-phenomenon), and by the fact that the six reviewers of this paper by Karnes et al, did not criticize these shortcomings. Let me proceed directly to a discussion of these points.

Failure to Conduct an Acceptable Replication Experiment

First, if one intends to replicate an experiment, then why depart from the specific successful protocol in ways neither necessary nor justified by any presentation of facts to establish a deficiency in the original protocol. The embellishment of Puthoff and Targ's protocol in which the "senders" ("outbounders" in Puthoff and Targ's terminology--why alter a descriptive terminology in favor of one that entails assumption of the particular "sender-receiver" mechanism?) take movies of the target site and make tape recordings of their impressions, while seemingly more "scientific" can easily adversely impact the ability of the judges

to cope with the extensive data to be judged. If the reader has never been a judge in such an experiment, this may appear to be an over statement. Transcripts of sixteen sessions each containing three or four pages of dialogue, an assortment of pages the judge must use to score targets if records are to be maintained in a complete fashion, movies, tape recordings and trips, repeated trips to the sites must be handled by the judges. And buried in all this material--if remote viewing is indeed a real phenomenon, there may exist no more than 5 bits (corresponding to a significance level of slightly better than 0.05 level of confidence) that hold the difference between success and failure to replicate. The use of large numbers of judges constituted a further deviation from the practice of Puthoff and Targ. While the use of more judges can reduce the chance of a type 2 error, this requires that the judges be unbiased. Moreover, there is the clear hazzard that the use of 64 judges will trivialize the experiment, which again Puthoff and Targ have stressed must be avoided. Why should the judges be biased or the use of 64 judges trivialize the experiment? I suspect an unreported experimental datum of Karnes et al is that these 64 judges are Karnes' students selected from his general psychology course taught at Metropolitan State College (Denver, Colo.). If such is the case, it is likely these judges had some awareness of their professor's expectations. Note in this regard Tart's (1980) rebuttal to Karnes et al. Tart points out that Karnes et al. did not follow proper experimental procedure that would assure isolation of the experimenter having knowledge of the target sites from the subjects. While Karnes et al. (1980a) are correct that the absence of positive results in their experiment argues against sensory leakage as to attitude or experimenter expectations must be assumed to have contributed to the experimental results reported by Karnes et al. That the authors do harbor and convey particular expectations is evidenced by the authors' presentation of their material. In their first paragraph proponent literature cited is a mishmash of both the scientific literature and such as Occult Medicine Can Save Your Life (1977), Vallee's The Invisible College: What a Group of Scientists Has Discovered About UFO Influences on the Human Race (1975), etc. The opposition side is characterized by, "on the other hand by the development of societies and journals concerned with critical evaluation of the paranormal..." It has long been an irritation among parapsychologists that their critics will not present legitimate parapsychological references but rather go to the drug store to seek out their opponents' definitive literature. Paragraph two sees "scientific proof." Paragraph three begins, "the scientific respectability claimed for remote-viewing..." And these cues to the reader as to the kind of objectivity with which Karnes et al approach their task of experimental replication continues throughout their report. To have stated that they did not obtain results corresponding to those obtained by Puthoff and Targ would have spoken to the issue clearly. The fact that an objective report has not been presented by Karnes et al. in any part of their paper, strongly argues that such bias probably existed during their experiment. As there was the opportunity for these experimenters to have influenced the subjects with information leakage, it must be concluded that the experiment reported by Karnes et al. does not represent uncontaminated results. As one cannot claim that the results are uncontaminated, one cannot claim this experiment as

reported by Karnes et al. represents a valid replication of the remote viewing experiments as reported by Puthoff and Targ. One must demand as high a standard for replication experiments as one would have required of the experiment replicated.

There is one further serious criticism to be leveled against the experiment reported by Karnes et al. The statistical procedures employed by Karnes et al. are not valid. The statistics are rendered invalid by a fault that has for quite some time become recognized by parapsychologists as a subtle but very serious hazard in conducting parapsychological research. This hazard enters whenever a less than optimum, other than standard, or multiplicity of statistical analyses are carried out on a single set of data. In the experiment of Karnes et al., the authors have altered the procedure for handling the statistical evaluation of the experiment from that used by Puthoff and Targ and thus open their results to the criticism of having sifted their data, as when they throw away and thus do not rank-order half of all the target-transcript sets. They state, "Judges were given the entire set of 16 receivers' protocols and were required to separate the 8 that best matched the target . . . Judges rank-ordered the 8 matches with 1 used for the best match through 8 for the least best match." This procedure was not used by Puthoff and Targ and thus represents data selection. Remember a $p < 0.05$ statistic would correspond to as few as 5 bits of information. That 5 bits could as easily reside among the 8 transcripts eliminated as the 8 variously retained by the 64 judges.

But that is not the only error in the handling of the statistics. Why, we may ask, select out the 8 best? We could also calculate statistics in which we select only the best 4, or the best 6, 10, 12 or retain all 16. Each procedure will yield a different statistic. The more ways in which there exist degrees of freedom in the selection of our statistic, the less meaning that one can attach to that statistical result. Suppose one were given a $p = 0.60$ statistic and told that it had been selected as the largest result out of 10 (while there are 16 possible selections of the type used by Karnes et al., some are less viable, as for example, if Karnes et al. had used 7 out of 16, readers might have felt this to be an odd number!) possible ways of constructing a statistic. What should be taken as the probable correct statistic? In the **extreme** case where each calculation is taken to be independent this would yield a corrected value of $p < 0.05$! In most cases as the results are not independent one would need to compute the statistics in every possible way to establish the range over which the statistic could have been selected. In the above example clearly no one can accept the 0.60 statistic as valid. Because of Karnes et al.'s handling of the statistics one cannot ascertain at this time what the correct statistic should be. One can state what the correct procedure for handling the statistics should have been. It should have been a complete rank-ordering; the complete rank-ordering has been used by Puthoff and Targ in the article referenced by Karnes et al.

Again, Karnes et al. failed to replicate the experiment carried out by Puthoff and Targ. Moreover, Karnes et al. have quoted statistics that have not been obtained by satisfactory procedures.

Causes of Non Replications Results

We have shown above that Karnes et al. did not conduct a valid replication experiment. Both procedure and data handling were significantly flawed. There are further causes for the results obtained by Karnes et al. that must be considered failures in experimental design over and above the fact that Karnes et al. failed to conduct a replication experiment. The above must be considered "sins of commission" in that procedures were used that altered the original protocol for no reason supported by justification. The following are what might be called "sins of omission," as they represent procedures omitted that should have been instituted in an effort to assure that, so to speak, the fisher was not throwing a net with mesh too large.

Hastings (1980) in his review of Karnes et al.'s paper gives the argument that the experimental results prove only that psi was absent in this particular experiment. This argument is not convincing. Indeed, Karnes et al. in their rebuttal state agreement and point out that Hastings is merely making appeal to the unprovability of the absence of something. Unfortunately, Hastings failed to quantify his argument. If one quantifies this argument it will be found that (1) the argument becomes sound and (2) the argument points to a failure on the part of Karnes et al. to design an adequate experiment.

A remote viewing experiment of the type Puthoff and Targ have conducted seldom yields results that read like an eyewitness account. Instead it is found that the transcripts must be evaluated by a judging procedure. This procedure and the overall experiment frequently yields results that are at the $p \approx 0.05$ level of confidence for unselected subjects (about which more will be stated below). Puthoff and Targ (1976) have reported insignificant results having been obtained with novice subjects. The experiment is designed to achieve a measure of the information transfer under the conditions of the remote viewing protocol. While frequently expressed in terms of the probability level of confidence, this is equivalent to a measure of the information transfer in units of bits of information.

Thus a 0.05 level of significance corresponds to somewhat less than 5 bits of information, more precisely 4.32 bits. Now this is the point. For an experiment having a mean value of 4.32 bits, the Poisson distribution yields a chance of 0.57 that replication will not yield significance. While Karnes et al. give us their statistic, they fail to calculate the probability that assuming existence of remote viewing as indicated by the results reported by Puthoff and Targ, their experiment would have failed to have detected its existence (under the assumption it is the statistical variability that is to be taken into account in the calculation). As several experimenters have replicated the remote viewing results as obtained by Puthoff and Targ, to be acceptable as contrary proof Karnes et al. should have to obtain a counter proof statistic less than that of the null hypothesis statistic established by the composite of the prior research work.

We should further demand that such a calculation be carried out under the additional constraint that Karnes et al.'s results are to be compared to those of Puthoff and Targ in which task naive subjects have been employed. The fact that Karnes et al. used subjects who said they were psychics is quite without significance. The proper

and the acceptable testing procedure as employed in parapsychological research is to treat subjects as novices unless experimental results have been obtained to indicate otherwise. Whenever a high scoring subject--"psychic"--is required for a particular experiment in parapsychology, one does not ask the subject. This is data to be established objectively. Proper testing procedure would require prior screening tests before accepting the subjects self designation as valid. Again, Karnes et al, evidence poor experimental procedure. Their experiment should, therefore, be considered as an exhibition of results to be expected under the constraint of inadequate techniques in which task naive subjects have been employed.

There is one final criticism of the work of Karnes, et al, and this criticism is perhaps of the most critical importance in efforts to replicate the experiments of Puthoff and Targ. One must design the experiment so that a bias on the part of the experimenter or on the part of the judges will automatically yield results that exhibit this particular flaw. In the experiment of Karnes et al, it is clear that this failure to replicate can easily be attributed to the ability or the inclination of the judges to incorrectly pair targets with transcripts, even assuming that sufficient information does exist in the transcript that would allow for correct pairing. Now when Puthoff and Targ conducted their experiment which yielded significant results, these results provided statistical evidence not only that remote viewing was operating, but that the judges employed were capable of detecting information cues in the transcripts designating the corresponding targets. Further, no control trials were necessary under these conditions as the a priori null hypothesis statistics are known. However, Karnes et al, do not obtain significance. This can mean either an absence of remote viewing information transfer or poor judging.

Poor judging must be eliminated by additional control tests. For a proper experiment, Karnes et al, would have to insert a number of transcripts under double blind procedures that have been specifically altered so as to contain adequate information to just correspond to the 0.05 level of significance (or about 5 bits of information) for an equal run of the simulated transcript--target pairs.

This writer finds it rather surprising that J. Calkins (1980) in his review of the article by Karnes et al, would have so castigated Puthoff and Targ for not running controls in an experiment that does not require controls as we know the null hypothesis probabilities a priori, while praising Karnes et al, for their design of an experiment that fails precisely because it does not incorporate this necessary, this vital element in its design. It should also be pointed out that this same error was committed by Marks and Kammann (1978) in their reported effort to replicate Targ and Puthoff's remote viewing experiments by using the original SRI transcripts.

In summary, I have enumerated seven critical inadequacies in the work of Karnes et al, any of which is fully sufficient to account for the failure to replicate. I find it incredible that these experimenters would have conducted work in such a fashion. I find it incredible that such a number of reviewers failed to point out and adequately justify all these points as none is that obscure. I can

only imagine that both groups are so engrossed in their particular views of the remote viewing research as to have rendered these otherwise excellent scientists somewhat less critical than they ordinarily are. It is to be hoped that Karnes et al and other researchers will conduct further replication experiments that are adequate to establish either positive or negative findings as regard the existence of remote viewing.

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EDWARD W. KARNES REPLIES TO SOLOMON E. FELDMAN, PIET HEIN HOEBENS, AND EVAN HARRIS WALKER:

Feldman's and Hoebens' comments relate to the point raised by Tart (1980) concerning the possibility of inadvertent or advertent experimenter cueing. We acknowledge that had positive results been obtained, those results would not have provided definitive support for a remote viewing hypothesis. The sensory leakage possibility cannot, however, be reasonably used to account for the obtained negative results. We are currently conducting another investigation of remote viewing addressing the adequacy of judges' hypothesis. The possibility of sensory leakage is being eliminated, and the results of that study will more clearly allow acceptance or rejection of a remote viewing hypothesis as suggested by Feldman.

Hoebens also commented on the possibility of purposeful cheating by subjects to provide spurious evidence for remote viewing since subjects were allowed to self-select themselves into sender-receiver pairs. In our previous studies of remote viewing (Karnes and Susman, 1979, and Karnes, Ballou, Susman, and Swaroff, 1979), self-selection of subjects was not allowed, and very rigorous procedures were used to avoid any possibility of fraud. The results of those studies yielded

no support for a remote viewing hypothesis. We, therefore, wanted to provide our experienced subjects every chance for success by allowing them to self-select their partners on the basis of their previously perceived successes in psychic communications.

We had considered the possibility of prearranged scenarios when we decided to have the senders record their impressions at the target sites. We had also decided on a course of action to evaluate that possibility if the initial group of judges were successful. In that event, two additional groups of judges would have been used. One group would have evaluated the receivers' protocols by visiting only the target sites; the other group of judges would have used only the senders' narrations. Success by all three groups would have supported remote viewing; success by only the initial group of judges and those using the senders' narrations would have supported a sender-receiver collusion hypothesis.

Walker offered a lengthy review of our experiment by citing perceived problems in the design and execution of the experiment. First, he stated that the records of the senders' experiences (movies and narrations) could have adversely affected the judges' ability to evaluate the data. We disagree. Each judge was given one sender's narration, viewed one film sequence, and visited only one target site. Each judge then studied the 16 receivers' protocols but these varied in length from one paragraph to no more than three pages of double-spaced type. The judging task was far from overwhelming as suggested by Walker.

Second, Walker suspects that our judges were biased against remote viewing because he suspects that the experimenters were so biased. We believe that they were not. They were solicited on the basis of having an open-minded interest in psychic matters and not from any of my classes. In fact, 62 of the 64 judges were totally unacquainted with me or my research assistants. I do share Walker's concern for bias possibilities in the judging procedure. In that regard, I suggest that he read Marks and Kammann's (1980) account of the multiple bias possibilities that existed in the judging procedures used in Puthoff and Targ's successful remote viewing experiments. None of those existed in our experiment.

Third, Walker claims that our statistical procedures were invalid--other than "optimum" and "standard", and that a multiplicity of analyses were performed. That criticism is totally without merit. Two analyses were used to evaluate the judging accuracy, a z test for proportions and a Student's t test. Two other analyses were used to evaluate the efficacy of feedback, a t test and an F test. These were quite optimum, standard, and few in number.

Walker appears to be totally confused when he offers the criticism of our having "sifted" or selected the data. The judges were, indeed, required to "sift" through the receiver's protocols, but there was no data selections by the experimenters. I share Walker's concern about data selection by the experimenters, and I would again suggest that he read Marks and Kammann's (1980) account of that possibility: "We have, therefore, found evidence that Targ and Puthoff selected the nine experiments published in the Hammid series from a larger set of experiments....obviously, if experimenters choose which data they publish, their findings become totally meaningless" (p.35). There was no data selection by the experimenters in our study.

Walker also appears to be confused about the proper statistical analysis appropriate for handling the fact that we required the judges to identify the 8 protocols that best matched the target from the entire set of 16. That procedure was used to evaluate the probability of a "hit" in the judging procedures. The a priori probability of a hit in our procedure was $8/16 = 0.50$. If we had required the judges to select the 4 of the 2 that best matched as posed by Walker, the a priori probabilities would have been $4/16 = 0.25$ or $2/16 = 0.125$.

Fourth, Walker claimed that our subjects were not screened and, therefore, somehow inadequate. That criticism is quite without significance since Puthoff and Targ (1978) state: "We have...carried out successful remote viewing experiments with about twenty participants, almost all of whom came to us without any prior experience... So far, we cannot identify a single individual who has not succeeded in a remote viewing task to his own satisfaction" (p. 90). Also, the apparently successful replication of remote viewing in laboratories other than SRI have used both experienced and inexperienced volunteer subjects. For example, Dunne and Bisaha (1979) used 19 untrained volunteer subjects in their mostly successful precognitive remote viewing studies.

Finally, Walker somehow equates the level of significance obtained in successful demonstrations of remote viewing to "bits of information" in the receivers' protocols. Unfortunately, he provided nothing in the way of how he arrived at this equation, and the significance of his discussion is unclear to me. If he is somehow arguing for an alteration in the level of significance, I would point out that the p. values obtained in our statistical analyses were nowhere near the .05 value that he cites. While it is true that one cannot accept the null hypothesis, the tenability of that hypothesis in our studies of remote viewing is enhanced by the facts that no statistical analysis reliably supported a remote viewing hypothesis, and that approximately half of all the analyses were in the direction of support while the other half were in the opposite direction.

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CHRISTOPHER SCOTT REPLIES TO JOHN BELOFF AND SYBO SCHOUTEN
(ZS, #6):

In my contribution to this controversy (Scott, 1980), I argued that the evidence for psi is essentially historical, being based on experiments that are unrepeatable, and that in these circumstances the weight of the evidence rests heavily on the experimental report. I pointed out that reports may be distorted or inaccurate and that this must be taken into account in evaluating the need to postulate psi as an explanation of the observations. In their rejoinders neither Beloff nor Schouten address this issue. It so happens, however, that their replies provide me with a neat demonstration of the point I was making.

Beloff first. He begins his rejoinder with the statement: "At least two of my critics (Cohen and Scott) have rebuked me for claiming that my seven experiments together 'represent an overwhelming case for accepting the reality of psi.'" No such rebuke is to be found in my paper. He goes on to say: "my opinion does not depend on any 'leap of faith' as these critics aver." The words in quotes do not appear in my paper, nor anything like them. Indeed nowhere in my paper do I speculate on Beloff's mental processes. I do criticize parapsychologists in general for jumping from the position "I can't think of any alternative explanation" to "there is no alternative explanation." But I would no more term this a "leap of faith" than any other piece of weak reasoning; essentially it arises from a lack of realism about one's personal fallibility.

Finally Beloff says he takes it that I hold the view that I "know in advance that psi is impossible." This is not my view and it was certainly not the view expressed in my paper. The whole tenor of my paper was to emphasize uncertainty. "How can we be confident one way or the other?" was my penultimate sentence.

Many people believe that scientists are taught to be accurate and, though liable to the odd slip like the rest of us, do not commit gross errors or radical misrepresentation of the facts. Beloff's performance here seems to relieve me of the need to offer further evidence against this mistaken belief.

Turning to Schouten, the situation is more complex but illustrates very well the kinds of difficulties we encounter when the whole weight of the evidence has to be borne by the experimental reports.

Failure to mark the start of a trial is a defect of the Brugmans experimental procedure which I noted as offering an opportunity for biased recording. Schouten replies that the onset of the trial was signaled by striking the floor with a hammer. But this information does not come from any of the contemporary reports, none of which makes any reference to the matter. It appears in an article published 18 years later by another psychical researcher (Carington) who had nothing to do with the experiments but who went to Holland in 1937 to visit the location of the Brugmans research. Carington

(1938) does not say how he got the information about the hammer being used. Moreover he does not say explicitly that the hammer was used to signal the start of a trial; it is possible (though less natural, I admit) to interpret his statement as meaning that it was used only to signal the start of the session. It would also be quite plausible to suppose that his informant meant this and Carington misunderstood him. Thus the available report by no means rules out the possibility that there was no signal at all for the start of a trial, and in this case the scope for biased recording of guesses would be much enhanced.

Even more crucial is the question of how the subject indicated his guess. The original report cited by Beloff does not say anything about this (Brugmans, 1922), but a later report (Brugmans, 1923) says that the subject signaled by tapping twice with his finger. I suggested (entirely speculatively) that the report might be "slightly mistaken on this one issue" and that, if so, this could make a crucial difference, leaving the door wide open to biased recording. I confess I did not suspect what a good guess this would prove to be. Thus, not only is the tapping signal not mentioned in Brugmans (1922), but it has now come to light that an earlier report by the three experimenters (Brugmans, Heymans and Weinberg, 1921) states "...as soon as he felt he had reached the right spot he pressed his finger down."¹ Obviously, observed from some 4 m. overhead, this signal would be far less sharp than the double tap mentioned in the later report. Incidentally, Carington in 1945 mentions neither tapping nor pressing down, but pointing. (If Carington was wrong on this he may have been wrong about the hammer.) Summarizing: the double tapping signal, crucial to clear observation of the subject's guess, makes its first appearance in the third report of the experiments, published three years after their occurrence, and contradicting the earliest report which refers to "pressing down."

The third argument that Schouten uses against my hypothesis of recording error is that there was no significant difference in the scoring rates when the three experimenters acted as agents. Of course this argument makes sense only if the agent was indeed the recorder, as I suggested. We do not know this for certain. (Beloff is inaccurate again on this.) Schouten and Kelly (1978) write: "According to the report, the agent selected the target square and the observers were not explicitly informed about the target until after completion of the trial." The wording seems to leave the door open to a new hypothesis: that the same one of the experimenters always acted as recorder and always knew the target while observing the subject's guess (even if he had not yet been "explicitly informed" of it).

¹ I am grateful to Mr. Piet Hein Hoebens for drawing my attention to this report and its contents. The distinction between tapping (Dutch tikken) and pressing (Dutch drukken) is exactly the same in Dutch as in English. The statement in this earliest report seems to have been overlooked by all subsequent writers on the Brugmans experiment.

This hypothesis would answer Schouten's objection since rotation of the agents would have no effect on recording.

But putting this hypothesis aside, is it a fact that all three agents scored the same? In the second report (Brugmans, 1922) we read (p.26-27) that one of the three experimenters, who was myopic, had almost no success in the two-room set-up although he scored well when in the same room as the subject. Why the discrepancy with Schouten and Kelly? Again this is a reporting problem. Brugmans confines his attention to the first seven sessions, comprising 187 trials, and indeed these are the only ones on which we have a contemporary report. The complete experiment covered 24 sessions and 589 trials; the 1978 article by Schouten and Kelly covers all of these, being based on the original worksheets. The finding that all three agents scored similarly is based on this full analysis, pooling all sessions and pooling the one-room and two-room conditions. Presumably the difference noted by Brugmans in 1922 has been swamped in these more extensive data. But which is more relevant? In the same-room condition it is granted that sensory cues were not eliminated, but it is argued that since the scores were no higher than in the two-room condition this is not crucial. As for sessions 8 - 24, it must be said that we have no experimental report in any normal sense, but merely an analysis of results; we have to assume that the conditions were the same as in the first seven sessions. In defense of this pooling, Schouten and Kelly argue that the results were no better (in fact they were much worse) in these later sessions. Clearly we are on shaky ground. What is the Brugmans experiment? The first seven sessions, in which case Schouten's argument is incorrect, or all 24 sessions, in which case we have no experimental report for the bulk of the work?

Schouten's fourth and last argument is that "assuming that the experimenters had a tendency to rate a near miss as a hit" there should be a deficiency of misses on squares adjacent to the target, and this is not observed. If this argument is considered valid then so should be the following: "assuming that the recorder had a tendency to rate an ambiguous hit/miss as a non-ambiguous hit there should be a deficiency of guesses classified as ambiguous and falling on the boundary between the target and an adjacent square, and this is observed." Indeed this effect is very marked: among the clear misses no less than 36 per cent of the guesses are explicitly classified as ambiguous, but among the clear hits and the ambiguous hit/misses only 8 per cent are so classified. This could be cited as providing strong evidence for motivated recording error. Schouten and Kelly remark on this finding and are able to show that it cannot account for more than about one third of the hits even if the effect is entirely due to biased recording. They also provide a number of alternative interpretations of the effect, based on various models of the subject's behavior. This is fair enough, but by the same token I can provide alternative interpretations of the effect they quote--that is, the absence of a deficiency of near misses. Here are two examples: (1) It could be that the biased misrecording of guesses arose not from rating near misses as hits but from misinterpretation or mal-observation of the subject's response signal (the double tapping, pressing down, pointing, or whatever). The subject may have moved

his hand from place to place over the board, frequently stopping and giving an ambiguous response signal, whose interpretation was influenced by the bias of the recorder. (2) It could be that the deficiency of near misses occurred but was compensated by an excess of near misses caused by the same factor as that which caused the hits. If the hits were caused by biased misrecording, then we only have to assume that the recorder believed that the objectives of the experiment would be supported by the occurrence of near misses, as opposed to far misses. I cite these two hypotheses not in order to advocate them (though I am, I confess, rather attracted to the first) but because they show how easily one can imagine circumstances, or "models," which would invalidate Schouten's argument.

Summing up, the Brugmans set-up has at least four possible defects on which the evidence is either non-existent or unclear.

- (1) The person recording the response may have known the target. (All reports silent.)
- (2) The start of the trial may not have been clear. (Only report is secondhand and 18 years after the event, and is not fully explicit.)
- (3) The response signal may not have been clear. (First and third reports conflicting, second silent; signal described in first report seems likely to have been unclear.)
- (4) The basis for classifying a response as "ambiguous" or not may have been subjective. (All reports silent; analytic evidence suggests heavy bias in classification - although alternative interpretations are possible.)

I suggest, as an alternative to psi, the hypothesis that the recorder (possibly one person throughout) knew the target and allowed himself to be influenced by this knowledge, taking advantage of the ambiguities (2), (3), and (4) listed above. This is the best non-psi hypothesis I can think of, but it is not necessarily the best there is.

I also suggest that a research with such defects in it does not merit the description "fundamentally sound" (Schouten and Kelly, 1978), though it may well merit inclusion among the seven best in parapsychology.

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JOHN BELOFF REPLIES TO CHRISTOPHER SCOTT'S REPLY TO BELOFF AND SYBO SCHOUTEN:

For some reason, my friend, Christopher Scott, seems determined to pick a quarrel with me where none exists. We are in agreement, surely, that (a) given the low level of repeatability in parapsychology everything has to depend on the reliability of the reports in question and (b) since no report can be exhaustive and few are completely accurate, it is always possible to think up some alternative normal explanation to account for the facts, as Scott, himself, demonstrates so well in the case of the Brugmans experiment. Where we do differ is in our respective judgements as to the plausibility of the psi hypothesis. I am happy to accept Scott's assurances that he does not regard psi as an a priori impossibility, but he would not, I think, deny that he regards it as so unlikely that any normal explanation, provided it is consonant with the facts, must take precedence over any paranormal explanation, a proposition that I cannot accept.

He does not, it is true, use the expression "leap of faith"-- the words are Cohen's-- and I ought not, therefore, to have coupled these two critics of mine in this way. Nevertheless, if his original contribution was not an implied rebuke to me for jumping to unwarranted conclusions (whatever my mental process, be it lack of imagination or a leap of faith), then I do not know what he was talking about. Why, therefore, in response to my mild rejoinder, he now sees fit to accuse me of "gross errors or radical misinterpretation of the facts," I am at a total loss to understand.

On the Brugmans experiment, I am hoping that my friend, Sybo Schouten, will be able to deal with the points which Scott now puts forward having made a much more thorough study of the relevant material than I have done. But, in the meanwhile, there is one aspect of that experiment to which I

would like to draw attention as it is much harder to reconcile with Scott's "misrecording" hypothesis. The experiment was carried out not just to demonstrate the existence of psi but also to compare performance under different conditions. In particular, the claim was made that there was a considerable (and statistically very significant) improvement in the scoring rate in those sessions after the subject had been given a small quantity of alcohol. Now, on Scott's hypothesis, no such differences should have arisen -- it was, after all, the subject, not the observer, who had taken alcohol. Consequently, to meet this evidence, Scott would have to suppose that the observer deliberately cheated. This, of course, is always a last resort for the skeptic, but I have the impression that Scott was here trying hard to avoid being driven to it. And there are other such differential effects in the data that are no less puzzling on the Scott hypothesis, for example the fact to which Irvin Child drew attention that there was a highly significant excess of direct hits for the shorter response times (see Schouten & Kelly, 1978, pp. 279-281).

JOSEPH AGASSI REPLIES TO ANDREAS N. MARIS VAN BLAADEREN RE "SUPERSTITION" (ZS #6 AND 3/4):

There is a sociological and anthropological tradition which takes alien beliefs--especially but not only among preliterate--seriously, and for various reasons and in various ways. One way is to relate superstition to some sort of primitive sociology, e.g., the tailoring of witch doctoring to social class (Robin Horton) and similarly, van Blaanderen observes, the influence of birth on career opportunities. Indeed, the very fact that a horoscope is thrown for a newborn infant has a high correlation with its high social class or, alternatively, with high incentives towards so upward social mobility. I agree with all this to a sufficient extent to accept it in the present comment.

I am distressed that my commentator ascribes to me the view that we possess only one item of certain knowledge, or rather almost certain knowledge, and that is that we are all ignorant, that there is no certainty or almost certainly no certainty-substitute. And I did not say "certainties can only be reached through a process of (Socratic) dialogue"; I think I said, rather than seek certainty, let us engage in (Socratic) dialogue. My commentator asks whether I succeed in offering "good solid advice to avoid ignorance" I have only one good, solid advice, I think: learn to live in ignorance since it is unavoidable.

My review of Recent Advances in Astrology was not to my commentator's taste; nor is to mine. It has not achieved its aim, I am told. I am convinced of that: even my most basic presupposition, stated in the previous paragraph, has been misconstrued by my commentator. I hope the patient editor permits this brief restatement.

The search for certainty is a hopeless task. Science is presented as the outcome of such a task, but in a most confused manner. Superstition is less of a body of doctrine, especially amongst the sophisticated, especially today, and more an alternative method of the pursuit of certainty. The scientist, however, sticks out his neck, states views, and tests them; the superstitious half-states his views and seeks for tests that will help him state his views better without risking refutation. Hence, in debates between scientists and superstitious, the superstitious appears illusive to the scientist, and the scientist appears cocky to the superstitious; regrettably they are both right all too often.

GEOFFREY DEAN ADDS TO ANDREAS N. MARRIS VAN BLAADEREN'S STATEMENTS ON ASTROLOGY (ZS #6):

It is a pity that Van Blaaderen's analysis of astrology in the context of superstition has obscured two important points, namely the distinction between popular and serious astrology, and astrology as a possible paranormal phenomenon. These points have wide implications which are seldom recognized and hence are worth discussing. But they are not especially relevant to superstition, hence what follows is not a critique of Van Blaaderen's analysis.

Popular astrology vs. serious astrology

Popular astrology is the highly visible outpouring of the mass media and is debunked by scientists and serious astrologers alike. Thus Van Blaaderen's distress at the popular belief in astrological determinism would be shared by most serious astrologers, as would his arguments against popular astrology. The problem is that such arguments are directed against topics which, although relevant to popular astrology, are not relevant to the way in which astrology is actually used by serious astrologers.

For example, in a typical consultation the astrologer will use the client's birth chart to throw light on whatever the client is interested in. This may be the kind of everyday concern that any counselor might meet, such as self-understanding, personal growth, making sense out of worldly chaos, making a job decision, or understanding a relationship; or just curiosity about astrology. The end result is normally that the client sees validity and meaning in what the chart indicates. And this is the type of evidence for astrology that astrologers respect most: it helps people, it works. In the astrological literature there are even occasional pro-astrology testimonies from counseling professionals. For example, the psychiatrist Dr. Edward Askren (who was once a sceptic of astrology) comments

Astrology...helps me see things I would not see otherwise
...and has helped me better grasp the issues of diagnosis
and treatment. Astrology is an excellent diagnostic tool;
that is, it increases my awareness of what is.

He adds the important rider

However, astrology is not treatment. (It is like) psychoanalysis and transactional analysis, both of which are theories of personality but not treatments as such. The treatment of persons must be learned by the individual practitioner through supervised apprenticeship over a long period of time.¹

Clearly, if such results are genuinely due to astrology then there is something here worthy of attention. But are they genuine? It is seldom recognized (least of all by astrologers) that such results could be due to non-astrological factors such as Barnum effects, placebo effects, and a host of others well-known to students of Cold Reading²; in other words to factors which allow us to perceive

meaning where none exists.

So what is the explanation? The answer is that nobody really knows because no proper studies have been made. My own limited tests have given mixed results, but they have not disproved astrology. I suggest that it is to counseling that critics of astrology should direct their attention, and that it is largely trivial to continue tilting at popular beliefs. In particular, it would be useful to know whether astrologers are any more or less helpful than other therapists; they are certainly cheaper.

Appropriate tests need not be difficult. For example, a particularly telling but very simple test is described by Hyman². He reports that in his early days he was an accomplished palmist and fully believed in it--until he tried giving readings exactly opposite to what the hand indicated, and found them just as acceptable. Such a test is easily applied to astrology. Naturally it will mean nothing unless the readings are highly specific (i.e., pruned of Barnum statements) and equated for both social desirability and occurrence, and the subjects are selected by personality test so that appropriate specificity is possible. Care should of course be taken to obtain the most suitable astrologer; astrology covers such a wide variety of approaches, and is unfortunately so open to exploitation, that to pick the first available practitioner could be disastrous.

Astrology as a possible paranormal phenomenon

A number of writers, e.g. Vaughan³, have suggested that an astrologer's successes are psychic rather than astrological. This idea can be greatly extended. I suspect that astrology shares a common theme with UFOs, Big Feet, synchronistic events, levitation, clairvoyance, predictive dreams, and so on, namely they seem to work for some of the people some of the time, but never enough to provide adequate hard evidence.

The emerging view (as suggested in this journal and elsewhere) is that this theme is characteristic of the paranormal, which (as suggested by Lyall Watson⁴ may be a product of our unconscious or collective unconscious. If this is true then an "explanation" of astrology becomes easy: the planets do not affect people, but some people (even before birth) may unconsciously apprehend planetary positions and symbolically organize their life to suit, just as the buyer of a tram ticket whose number is synchronistically the same as the telephone number he is about to ring may have unconsciously apprehended the appropriate moment to buy (or alternatively to ring).

It would also follow that people for whom astrology works should tend to see UFOs or experience synchronistic events or have precognitive dreams or whatever, more than those for whom astrology does not work. At first sight this may seem to be saying no more than multiple delusions are more frequent among those already deluded; except that astrology, unlike the rest, can be tested on demand by a simple comparison of birth chart and life history.

Also, unlike most of the rest, the test can be made objective and difficult to falsify; thus one can no doubt explain away a UFO sighting but how does one explain away a life which follows planetary movements?

Is there any evidence to support this hypothesis? The only astrological effect that has been reliably established to date is Gauquelin's Mars Effect. An essential requirement for the Mars Effect to manifest seems to be prominence; only the most prominently successful exhibit the Effect, and the less prominent do not. And prominence seems to be related to better-than-average precognitive powers, at least in business executives.⁵ If the connection is real, it not only supports the present hypothesis but suggests a possible solution to the mystery of the Mars Effect--plus obvious strategies for further investigations.

In summary, I am suggesting that astrology could introduce a long-needed element of on-demand testability into the paranormal mix. So perhaps here is a straw which parapsychologists should consider clutching at more than they have been doing.

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JON BECKJORD RESPONDS TO ROBIN RIDINGTON RE SASQUATCH "PHOTOS" (ZS #6):

In response to Ridginton's reply to my comments in ZS #6, I must admit that it is unfair to expect him to agree with my evidence if he has not seen it. The basis for his objections to my para-physical theories regarding the existence of sasquatches is that he doesn't believe that I and others have actually photographed animals or beings that we could not see at the time the photographs were taken. Since he has not seen these photos, but only drawings from them, he cannot be blamed for being skeptical. Hence, Robin Ridington is invited to visit me in Seattle, which is close to Vancouver, to view the photos and judge for himself. They were mostly taken after the UBC Conference in 1978, so he has had little chance to see them. If he does see these in person, he will be given the chance to join a distinguished group of persons who have seen them

and who do agree that some, if not all, of the figures in the photos are indeed animal figures. Robert Sheaffer, a skeptic and a Fellow of the Committee for the Scientific Investigation of the Claims of the Paranormal, has viewed them, and has recently admitted in print (Saucer News July 1980) that he can see one sasquatch ("Big Mama") in one of the photos. Kendrick Frazier of the Skeptical Inquirer has admitted in a telephone conversation to me that he can also see this same figure, although he is not yet ready to admit it is a sasquatch. The four off-duty Army photo-interpreters mentioned in my Reply article have acted again as consultants for me, and after seeing in stereo and normal modes the photos taken last summer and previously in the Sierras, have stated that they can indeed see "apes," "monkeys," "ape faces," "a gorilla," "gorillas," "a cat-like face," "a dog," "dog-like animals," and "there is definitely something there that we can't explain away."

Ridington writes that my challenge to Science requires more solid evidence than mere claims to seeing animals in photos that weren't normally visible. Yet, if scientists are able to see these animals, and do come to agree that they weren't visible at the time of the photography, then is this not indicative of something truly of an earth-shaking nature for Science? We lack, and I am convinced, will continue to lack, solid evidence such as bones. I suggest that we forge ahead and continue to seek alternatives and methods that will enable us to measure this phenomena, which continues, day after day after day. Ridington's skepticism relates in a way to the findings of Richard Greenwell in the same issue of ZS. When reading Greenwell's article, I wondered how many of the scientists polled, including critics like Ridington, had ever actually gone out into the field (i.e., the woods) to follow up on alleged sasquatch reports? And how many articles and how many books had such scientists actually read (not skimmed) on sasquatch research? I would suggest that critics such as Ridington would benefit greatly from at least some field exposure prior to writing their criticisms, and at a minimum, they would, or should be exposed to the Patterson Bigfoot Film, which I now must admit Ridington was not exposed to in the manner I described. This was my error, based on a two year old memory of the Conference, but I speculate that if Robin Ridington had attended the film-showing session in Dr. Coon's rooms, that his view of the sasquatches would be greatly different today. The invitation is still open to Ridington and to his colleagues to visit and see all these materials.

BOOK REVIEWS

Para Psi und Pseudo: Parapsychologie und die Wissenschaft von der Täuschung. By Lutz Müller. Berlin, Frankfurt, Vienna: Ullstein, 1980. 256 pages, illustr. 34 D.M. (About \$18.)

Reviewed by Piet Hein Hoebens

Although the "natural antagonism" between the two groups has often been exaggerated, the relationship between the parapsychologists and the magicians has traditionally been an uneasy one. This is not surprising. Psi-phenomena and magic tricks have the somewhat embarrassing habit of looking like Tweedledee and Tweedledum and disputes over "who is competent to judge what" are bound to arise. Some parapsychologists resent what they perceive as the meddlesomeness of psi-investigating illusionists, whereas some magicians suspect behind every parapsychologist a credulous dupe who, rather than listen to the experts, invokes quantum physics to explain why handkerchiefs can turn into pigeons. Researchers who may claim to be insiders in both groups are relatively rare.

Dr. Lutz Müller from Stuttgart, Germany, is one of those exceptions. A professional psychologist who has long been associated with Bender's psi-institute at Freiburg University (he received his Ph.D. on a parapsychological subject), Müller is also an experienced magician, a member of the German Magic Circle and a frequent contributor to magic journals.

Para Psi und Pseudo (the title behooves no translation) is a revised and enlarged version of his 1977 doctoral thesis, devoted to "parapsychology and the science of deception." As the language barrier will prevent many readers of this journal from personally acquainting themselves with this excellent book I will attempt a rough summary of its contents.

Müller regards the history of psychical research "as a history of the confrontation with trickery and fraud." From this assumption it logically follows that parapsychologists must seek close cooperation with experienced magicians. Such an interdisciplinary approach is the only way to avoid repeating the embarrassing mistakes of the (often very recent) past.

After an amusing survey of the most notorious cases, Müller severely criticizes the naive belief of psychical researchers like the German Gerda Walther that genuine paranormal phenomena can be recognized by their self-validating genuineness. The perfect deception, he asserts, is indistinguishable from the real article, as the subtle introduction of psychological elements that cause the observer to declare the phenomenon authentic is part of the trick. The only criteria for genuineness are "those criteria that exclude cheating." It is obvious, according to Müller, that only the real experts on trickery - the magicians - can with any degree of confidence decide whether these criteria have been met in a given experiment. Müller

proposes a standard "deception analysis" as an essential preparation for experiments with supposedly gifted subjects. In this analysis, various aspects of the procedure must be inspected for the "degree of freedom" they allow the subject. For example, if a subject insists on being tested in familiar surroundings, refuses to be searched for gimmicks and requires total darkness, the "degree of freedom" must be judged "high" on those three points, and a priori suspicions of fraudulent intentions must be commensurate--regardless of how "reliable" the fellow looks. The main part of the analysis is to decide in what way the special conditions prevailing in a given experiment could be taken advantage of by a trickster. The average scientist, Müller sadly notes, is singularly ill-equipped to perform such an analysis without the assistance of an experienced magician. Obviously, cooperation is called for. Yet trick-experts have relatively seldom been consulted in parapsychological experiments. Strong prejudices on both sides often prevent a fruitful contact. Many parapsychologists have misgivings about the "inhibiting influence" that presumably would result from the presence of a magician. On the other hand, many magicians have a priori reservations of a different kind. Müller cites the embarrassing instance of a French illusionists' congress in November 1976 where magicians who testified to their having observed apparently paranormal metal bending were all but shouted down by angry colleagues. However, Müller sees some signs of a growing ecumenism. The Freiburg Institute has invited magicians to serve as consultants. Utrecht parapsychologist Martin Johnson has urged his colleagues to cooperate with the Magic Circles (and in 1976 the PA conference in Utrecht organized a magic demonstration with the embarrassing result that quite a number of leading parapsychologists started suspecting the magician of real paranormal powers).

A poll taken by Müller among the members of the German magic community suggests the trick experts are by no means as hostile as is often assumed. Although 81.6% thought ALL Geller's feats were tricks, 72.3% said they thought psi was probably a real phenomenon. The respondents, however, were self-selected and only 283 of the 1000 invited sent back their questionnaires, so Müller presents his findings with utmost caution. The questionnaire method, moreover, did not allow him to look for possible correlations between degree of belief and degree of competence, which could have yielded interesting results.

In a short, but important chapter on "Understanding Cheating," Müller places the issue in a wider context. One of the most fundamental and pernicious deceptions, he states, is the idea that deception is a marginal phenomenon, an isolated disruption in an otherwise "objective" view of reality. He insists (as Hansel did) that cheating (including self-deception) is a vital ingredient of human psychology, a natural result of the way the mind perceives its environment. Müller regrets that this important aspect of human existence has not yet become the subject of a special "Science of Deception," although the magical brotherhood has long been practicing an underground version of such a science.

Psychical researchers have often become the victims of hoaxers and tricksters, Müller states, not only because they knew too little about the technical possibilities of trickery, but even more because they greatly overestimated their own competence as observers, and underestimated their unconscious willingness to be led astray.

From the arguments Müller advances and from the fascinating examples he has chosen, the determined hard line skeptic could construct a strong attorney's case against the pretensions of parapsychology in general. If so little is known about the psychology of deception, then how can we hope realistically to estimate the importance of the fraud factor in psychical research? If even Podmore could be tricked, then why should we put any trust in the judgment of persons less perspicacious than Podmore? If a number of Houdini's tricks are still unexplained, then how can we ever attempt to deduce the authenticity of "paranormal" events from their inexplicability?

Müller, however, is no hard line skeptic. From the perspective of the extreme critic, he would even qualify as a "believer." In the introduction to his book he states his view "that, by and large, the existence of psi-phenomena can be regarded as having been scientifically demonstrated." While discussing pseudo-psi he often seems to take the existence of the genuinely paranormal for granted, even where his own arguments would seem to throw doubt on even the best evidence.

It is my feeling that Lutz Müller has not been entirely successful in solving his own identity-problem as an observer of the paranormal scene. The book at times reads like an uneasy compromise between the parapsychologist Jekyll and the trick-expert Hyde. Jekyll approvingly quotes Bender's attacks on the pig-headed skeptics, while Hyde almost out-Hansels Hansel in asserting the universality of cheating.

This ambiguity is probably responsible for the (very few) disappointments I experienced while reading the book. I had expected to find an extensive discussion of the poltergeist, which after all is one of the specialties of the Freiburg school. With his unique background as a magician and a former associate of Bender's institute, Müller would have been in an unrivalled position to throw some light on still controversial cases like the Rosenheim and Bremen RSPK outbreaks. Poltergeists, however, are not discussed at all.

Likewise, Müller is extremely reticent in reporting his own experiences during his stay in Freiburg. The reader is not informed about the experiments the author must have observed himself. Müller mentions the very interesting fact that Geissler-Werry, one of Germany's top magicians, has been a consulting trick-expert at the Freiburg institute. The reader would wish to know more about Geissler-Werry's experiences there. Did he ever watch Bender's star spoon-bender Silvio in action, and, if so, what did he think of it? The question remains unanswered in Para Psi und Pseudo. (In a personal communication, Dr. Müller notes that Geissler-Werry did attend at least one experiment with Silvio. No metal bending took place).

Müller's reluctance, critically to discuss that part of the parapsychological world to which he himself has belonged for years, however, should be seen within the context of the pro- and con- debate in Germany. That country used to have an admirable tradition of responsible skepticism vis-a-vis the paranormal, exemplified by Dessoir, Von Klinckowstroem and, to a certain extent, Gubisch. In recent years, however, skepticism in Germany has become increasingly identified with a small number of excessively hostile "disbelievers"-- notably Otto Prokop, professor of forensic medicine in Berlin, and the criminologists Herbert Schäfer and Wolf Wimmer. These critics work from the unquestioned assumption that the Laws of Nature are sacred and that parapsychology is nothing but medieval superstition dressed up as "science." The presence of a psi-institute at Freiburg University they see as one of the gravest threats to western civilization. Indeed, one of them (Wimmer), has managed to imply that Bender and his associates must share responsibility - in retrospect - for the witch hunts in the Middle Ages!¹ According to Wimmer, "fairness has its limits" when dealing with aberrations like psychical research. The fairness of these three skeptics certainly has its limits. English-speaking parapsychologists who complain about the English-speaking skeptics should read Wimmer and Prokop's book "Der moderne Okkultismus."² Then they will agree that, by comparison, James Randi is positively "sheepish."

Some passages in Para Psi und Pseudo to me suggest that Müller may deliberately have wanted to avoid providing Prokop, Wimmer and Schäfer with fresh ammunition for their almost hysterical campaign against parapsychology. If my hunch is correct, I would question the wisdom of Müller's protective attitude. Parapsychology is best served by total frankness about its weak points. (Incidentally, this seems to be the attitude adopted by the Freiburg Institute in recent years. Its journal, now edited by Eberhard Bauer, has increasingly become a forum for "believers" and "skeptics" alike. Critical re-evaluation of psi-claims is actively encouraged.)

Para Psi und Pseudo is an excellent book, but it might have been even better if the author had been a tiny bit less discreet.

References:

¹Wimmer, W., Hexenwahn an Universitäten?
Zeitschrift für Allgemeinmedizin Vol. 56 No. 22, August 10, 1980,
Stuttgart

²Prokop, O., and Wimmer, W., Der moderne Okkultismus.
Gustave Fischer Verlag, Stuttgart, 1976.



Messengers of Deception: UFO Contacts and Cults. By Jacques Vallee.
And/Or Press, Berkeley, 1979. 243 pp. Paper, \$6.95. Cloth \$11.95.

Reviewed by J. Richard Greenwell

Jacques Vallee is a French astronomer/computer scientist residing in California who has written several books on unidentified flying objects (UFOs). During the 1970's his writings, which advanced metaphysical rather than extraterrestrial explanations for UFOs, had a profound effect on "UFOlogy," and established an enthusiastic, worldwide following.

This new book is another attempt, with not altogether happy results, to consolidate his current thoughts. Many of these thoughts seem to have come from a mysterious "Major Murphy" ("although his actual rank is much higher"), a retired member of the intelligence community, whom Vallee befriended at obscure contactee meetings. The contactees, Major Murphy suggested to Vallee, are being deceived (thus the title of the book), and are being given confusing and contradictory versions of reality by the "manipulators," who are attempting to propagate a myth of extraterrestrial visitation. Major Murphy's eloquence seems to have impressed Vallee immensely, to the point where one may wonder, if there is any mystery at all, who is doing the manipulating.

Vallee proposes three scenarios to explain the UFO phenomenon. The first involves a highly-sophisticated British wartime intelligence group set up by Winston Churchill to confuse Nazi Germany, which Vallee mistakenly states built an Ultra cipher machine. (The correct version is that British intelligence quietly acquired a German Enigma cipher machine in Poland in 1939, which made possible its super-secret Ultra intelligence operation at Bletchley, and without which World War II may have been lost to the Nazis. Further machines were captured from German aircraft and warships in 1940 and 1941.) What became of this group after the war? Retirement on mediocre British civil service pensions? Replacement by a more modest peace-time operation? Nothing so mundane. According to Vallee, the Scandinavian Ghost Rockets of 1946 could have been "a continuation" of their deceptions, perhaps even using some captured German developments. The purpose: to simulate an extraterrestrial invasion in order to unify mankind and prevent another world war. Furthermore, Major Murphy, who also claims to closely follow U.S. government-funded research on parapsychology, assured Vallee that "silent, disk-shaped flying machines can be built." Such operations have continued to the present, in this scenario, and all the unexplained UFO reports can be attributed to the deception work of this group. Most Western intelligence spheres would be unaware of its activities, and such enterprises as Project Blue Book and the University of Colorado UFO Project (1966-68), were simply cover operations.

The infiltration of UFO groups by the CIA would be a part of this conspiracy, and Vallee suspects sinister CIA affiliations on the part of board members and advisors of such organizations (another idea from Major Murphy). These operatives are supposedly checking on both the general and specific activities and thoughts of UFO

notables; they are known in intelligence circles as the "vacuum cleaner" and the "little aunt" respectively, according to Major Murphy. Another tactic in this conspiracy would be the manipulation of the contactees, in order to both increase public belief in extraterrestrial visitation and to ridicule UFO reports, thus discouraging serious scientists from becoming involved and possibly realizing the deception at work. UFO debunkers and their journals (including, presumably, The Skeptical Inquirer) play a key role here, and "may have links to intelligence organizations."

The second hypothesis, labelled "esoteric intervention," is harder to define. Vallee postulates that an occult group may have discovered how to mentally project images, may have made contact with "other forms of consciousness," and may even know the real nature of UFOs. He finds merit in a proposition by Major Murphy that contactee cults could be fronts for an "influential group" on Earth, and that such a group could be moulding our collective future. That is, it is not the UFOs themselves such a group is controlling, but only the belief in them by a wide segment of the population.

Related to this is Vallee's belief that major but secret breakthroughs have occurred in psychopharmacology, enabling the total control of the human sensory, perceptual, and memorial functions. Major Murphy, it seems, showed him clippings from The New York Times in which secret government behavioral research was discussed. Many thousands of specialists in this country and abroad are currently working around the clock in numerous advanced areas of biochemistry, neurophysiology, neurosurgery, and psychopharmacology. Their discoveries and advances come slowly and painfully, and most of the areas Vallee implies have been mastered are far too complex for our current state of the art to decipher. To propose that a small group could secretly do what ambitious specialists at universities (where 52% of American research is conducted) and the National Institutes of Health cannot do, is absurd, despite Major Murphy and New York Times clippings.

The third hypothesis, clearly Vallee's favorite, is the "control-system" hypothesis. Here he feels that the UFO "represents a manifestation of a reality that transcends our current understanding of psysics," and that this reality "is larger and more complex than a simple visit by interplanetary travelers..." Vallee believes that we live in a "system" which transcends time and space, that humans are capable of understanding it, and that some humans already have. It is they, presumably, who are manipulating the belief in UFOs (not the UFOs themselves). He admits, almost reluctantly, that the system may have an extraterrestrial origin, although UFOs are not in reality extraterrestrial "nuts and bolts" vehicles.

Throughout the book, Vallee seems extremely preoccupied with the contactee movement, which he believes is being stimulated by one of our mysterious groups, and that such mystical irrationality could ultimately destroy the rationality and science we currently possess. We should remember, however, that societies need their cultural fringes (located at the extremes of social frequency distributions)

to remain vibrant. This phenomenon is well-known to sociologists, and there is no reason to believe that the contactee movement serves any other social purpose than that, that it is different from movements in other cultures at other times, or that it is growing at an alarming or disproportionate rate.

Also, while some state that the differences have become nebulous, there is a fundamental distinction between alleged contactee events and alleged UFO abductions. In the former, individuals voluntarily interact with beautiful, human-like "space brothers," receive a mystical revelation, are given a message and/or task, and are sent on their way. In the latter, individuals are taken "aboard" against their will by less attractive, non-human-like creatures, examined biologically, treated with indifference at best, and released; there is no message of salvation or task involved. Vallee ignores this distinction, and indiscriminately blends these two phenomena together without even an explanatory sentence.

A large part of the book is dedicated to Vallee's personal involvement in contactee lore, particularly in a group known as the Order of Melchizedek. Much of this material is included to support the absurdity of the contactee claims; not that the events did not occur (or were not perceived), but that a manipulating force must be behind the events. Two full chapters (almost 30 pages) are dedicated to animal mutilations, although it is not made altogether clear where they fit into the scheme of things. Vallee believes they may represent some kind of message to the authorities that all is not under their control.

Other "mysteries" are touched upon. Contactee George Adamski was believed to have travelled on a special U.S. passport, and had pre-war connections with American fascist leader William Dudley Pelley. The latter may have introduced Adamski to pseudo-contactee George Hunt Williamson, who knew John McCoy and pseudo-contactees Ray (UFOlogist) and Rex (parapsychologist) Stanford. In fact, McCoy, who linked UFOs to the Jewish Banker Conspiracy, and Williamson authored a book together. So did McCoy and the Stanford brothers when they all lived in the same Texas town. And as if all this were not enough, Vallee once took a cab in Los Angeles, and, upon examining the receipt a few days later, found, coincidentally, the driver's name to be Melchizedek!

What is the reader to make of all these things? Vallee states that "the only way to fight the confusion that surrounds the UFO problem is to realize that much of this confusion is deliberate." That may well be, but some readers may wonder whether this new book is not a further contribution toward such confusion. As for himself, Vallee claims that he is "beginning to perceive a coherent picture of the 'flying saucer' phenomena for the first time."

Several direct errors of fact were noted in the book. Perhaps they are minor, but such errors always serve as indicators of the care with which an author prepares his work. He states, for example, that the military UFO reporting regulations are still in effect.

However, Air Force Regulation (AFR) 80-17 was cancelled at the time Project Blue Book was terminated in December of 1969. According to the current Numerical Index of Standard and Recurring Air Force Publications (AFR 0-2), issued on June 15, 1979, AFR 80-17 now refers to the Air Force Independent Research and Development Policy Council. Furthermore, the Numerical Index lists no regulation concerning reporting procedures or investigation of UFO incidents. Vallee, a Californian, refers to Santa Barbara's Center for the Study of Democratic Institutions as the Center for Democratic Studies, and erroneously labels it a "think tank" (it is actually a center for the intellectual discussion of pressing social issues). Even CUFOS, founded by Vallee's original mentor, astronomer J. Allen Hynek, is erroneously referred to once as the Center for UFO Study.

In an epilogue, University of Hawaii sociologist David Swift attempts to provide a reasonable condensation of Vallee's thoughts, without which the work would completely fall over the edge of reality. The book is also published as a quality paperback, which withstood this reviewer's many underlinings and comments. It has note references, a bibliography, and an index.



Guardians of the Universe? By Ronald Story. New York: St Martin's Press, 1980. 207 pages. \$8.95.

Reviewed by Morris Goran

Ronald Story's Guardians of the Universe? (St. Martin's, 1980) is essentially a revised version of his The Space-Gods Revealed (Harper and Row, 1976). The theme of picking apart the ancient astronaut idea is identical, and some of the major points are repeated. However, the new book is noteworthy for several reasons. Robert K.G. Temple's The Sirius Mystery is criticized, some new photographs are illuminating, the selected bibliography is lengthy, and the very sparse number of books against the ancient astronaut concept is increased by one.

The last fact is more than minor when considering the inroads made by ancient astronaut promoters. In 1979, a small educational publisher, J. Weston Walch of Portland, Maine, issued a paperback, Science Activity Reader by E. Richard Churchill and Linda R. Churchill, wherein Erich von Däniken is given the same treatment along with such figures as Galileo, Darwin, Madame Curie and Enrico Fermi.

Despite the positive aspects of Guardians of the Universe?, Story's first book is the better one. Guardians of the Universe? promises "an in-depth look at some of his (von Däniken's) important predecessors" (page 20) but in-depth turns out to be a few pages. The work of Alexander Thom is missing from the bibliography of archeo-astronomy while Edwin Krupp's book listed there is worthy of being included also in the section "sceptical of the ancient astronaut theory." British rather than American editions of books are often cited. Then, Professor Donald Menzel, godfather of the UFO debunkers is characterized as "overly emotional" when discussing UFOs and shooting off "his mouth too quickly" (page 142).

Guardians of the Universe? advocates the UFO phenomenon. Chapter 14 is titled "UFOs: A Genuine Mystery." Even the Appendix by J. Richard Greenwell dealing with Flindt and Binder's arguments against organic evolution has a final short paragraph proclaiming that UFOs are a separate discipline. On the other hand, Dr. Clifford Wilson, author of Crash Go The Chariots (Lancer Books, 1972) placed his UFO beliefs into a separate book, UFOs and Their Mission Impossible (New American Library, Signet, 1974).

Story's support of UFOs brings up the larger question whether one group on the borderland of science is against another such group not so much to expose a pseudoscience but for other reasons such as to push aside a competitor. The ancient astronaut idea gives more prestige to the spaceship theory for unexplained UFOs, and those who favor other hypotheses--life forms or different dimensions, whatever that may mean--are left out.

Earlier in this century, UFO enthusiasts including Adamski and Jessup could embrace the ancient astronaut idea. Today there is a chasm. Robert Temple writes disparagingly about UFOs.

Story is impressed that for UFOs "there still remains a residue of seemingly inexplicable phenomena (page 140). Yet what area of nature does not have much that is unexplained? Many of the UFO advocates have become instant experts on lie detectors, human perception, human personality, and interviewing in order to explain. Is an astronomy professor an authority on whether a person lies?

Story contrasts UFO and ancient astronaut "evidence" as unsolved and solved respectively, but the more sophisticated ancient astronaut believers--there are some--do not accept this categorization. Likewise the UFO believers do not accept the interpretations of Menzel, Klass, Taves, Schaeffer, Oberg and others.

The UFO believers have a much larger literature, monetary and emotional investment, and number of participants. Yet both "pseudosciences" have world-wide attention and well-educated supporters. Indeed, people with good credentials can be found in favor of every well-known, as well as many little-known, occult and pseudosciences. It appears that both the UFOs and ancient astronaut concepts are not going away to be buried. They are both here to stay awhile.



BOOKS BRIEFLY NOTED

- * Listing here does not preclude later full review.
- * Critical annotations are by Marcello Truzzi.

Bloch, George, translator and compiler, Mesmerism: A Translation of the Original Scientific and Medical Writings of F.A. Mesmer. Los Altos, Cal.: William Kaufmann, 1980. 176pp. \$11.50. A most welcome addition to the hypnosis literature. Recommended.

Brier, Bob, Ancient Egyptian Magic. N.Y.: William Morrow, 1980. 320pp. An excellent popular introduction by philosopher-parapsychologist Brier.

Brunstein, Karl, Beyond the Four Dimensions: Reconciling Physics, Parapsychology and UFOs. N.Y.: Walker and Co., 1979. 222pp. \$12.95. A fascinating mixture of ideas, including theological, by a physicist seeking integration in terms of a fifth-dimensional theory.

Bunge, Mario, The Mind-Body Problem: A Psychobiological Approach. N.Y.: Pergamon Press, 1980. 250pp. \$28.00. An important new philosophical analysis written most lucidly. Instructional even for those unlikely to agree with the author. Recommended.

Campbell, Bruce F., Ancient Wisdom Revived: A History of the Theosophical Movement. Berkeley, Cal.: University of California Press, 1980. \$12.95. An impressive scholarly work indispensable to anyone interested in this extraordinary movement. Recommended.

Catran, Jack, Is There Intelligent Life on Earth? Sherman Oaks, Cal.: Eidirhaven Books, 1980. 220pp. No price indicated. An attempt to "expose the myth of extraterrestrial intelligence" coupled with an attack on our free enterprise system's frustrating of science and technology's capabilities for solving our social ills. A perscription for a technocratic future.

Dames, Michael, The Avesbury Circle. N.Y.: Thames and Hudson, 1977. 140pp. \$12.95. A highly controversial but fascinating interpretive attempt to "reveal" the collective purposes of the great Neolithic achievement. A highly integrative and creative analysis.

Davis, Winston, Dojo: Magic and Exorcism in Modern Japan. Stanford, Cal.: Stanford University Press, 1980. 332pp. \$18.50. A study of Sukyo Mahikari, an exorcistic sect in Japan. An important sociological ethnography with serious implications for conventional theories of magic and religion since this group represents a "church of magic" that is remarkably compatible with the functional rationality of an industrial society.

de Mille, Richard, editor, The Don Juan Papers: Further Castaneda Controversies. Santa Barbara, Cal.: Ross-Erikson, 1980. 519pp. \$8.95 paperback. A remarkable sequel to de Mille's earlier devastating expose of the realities behind Carlos Castaneda's pseudo-ethnographies. This collection--in addition to further destroying the Don Juan myth--constitutes an excellent documentary history of the controversy that should be read by anyone seriously interested in the sociology of science and the current status of anthropology. Highly recommended.

de San, Maurice G., Hypothesis on the UFO Origin. Bologna, Italy: EDITECS Publishing House, 1978. 84pp. (A supplementary UPIAR Monograph.) No price indicated; paperback. A highly speculative but intelligent con-

sideration of extraterrestrial origins of UFOs as a result of evacuations following supernovae explosions.

- Eberhart, George M., compiler, A Geo-Bibliography of Anomalies: Primary Access to Observations of UFOs, Ghosts, and Other Mysterious Phenomena. Westport, Conn.: Greenwood Press, 1980. 1114+xi pp. \$59.95. A truly extraordinary compendium of geographically arranged 20,000-plus references to anomalies. It is unfortunate that the items are arranged geographically with the index being the means to look by topics --since most readers would probably have preferred a topical arrangement with an index giving geographical breakdown. Nonetheless, this volume is most welcome and obviously a massive labor of love. All anomalists should be grateful to Mr. Eberhart for doing the needed job. Highly recommended.
- Feldman, Anthony, Space. N.Y.: Facts on File, 1980. 336pp. \$19.95. A lavishly illustrated survey of astronomical knowledge and lore including much about space mysteries such as black holes, quasars, etc., and particularly well illustrated sections dealing with UFOs and exobiology. Very good introduction to the topics covered, especially for the juvenile reader.
- Fisher, Joe, and Peter Commins, Predictions. N.Y.: Van Nostrand Reinhold, 1980. 224pp. \$12.95. A very entertaining but not really very serious look at predictions from everywhere and everyone. Fun but not very discriminating among the soothsayers covered.
- Goran, Morris, Science and Anti-Science. Ann Arbor, Mich.: Ann Arbor Science, 1974. 128+xi pp. \$10.00. An exceptional work deserving of far more attention than it has received, which dispels many myths and looks at all aspects of the anti-science movement including the internal conflicts within science. Full of fascinating and often surprising facts, the book should be read by anyone interested in this topic. Highly recommended.
- Goran, Morris, The Future of Science. N.Y.: Spartan Books, 1971. 156pp. No price indicated. A very refreshing work upholding science but fully recognizing its human character. A wonderful antidote to the "Storybook" picture of science so commonly taught. Lucid, entertaining and highly informative. Highly recommended.
- Gould, Stephen Jay, The Panda's Thumb: More Reflections in Natural History. N.Y.: W.W. Norton, 1980. 343pp. \$12.95. A new collection of Gould's always entertaining and informative essays, including Gould's remarkable new conclusions about the Piltdown Man hoax.
- Grant, John, and Colin Wilson, editors, The Book of Time. North Pomfret, Vt.: David & Charles, 1980. 320pp. \$32.00. An interdisciplinary collection of seven original papers by specialists--from physical science, horology, anthropology, philosophy, and history--on the subject of man's relationship with time. Colin Wilson's essay "Time in Disarray" should be of special interest to those concerned with the paranormal.
- Hand, Wayland, editor, American Folk Medicine. Berkeley: University of California Press, 1981. 355pp. \$5.95 paperback. An extraordinary collection of engaging and enlightening scholarly essays -- 26, ranging from such topics as "Miraculous Restoration of Lost Body Parts" to "Birthmarks and Psychic Imprinting." This 1976 collection is a classic and its paperback edition is most welcome. Recommended.
- Hathaway, Nancy, The Unicorn. N.Y.: Viking Press, 1980. 192pp. \$19.95. A beautiful book, lavishly illustrated and impressively done. Well researched and includes an annotated bibliography. Recommended.

Howell, Michael, and Peter Ford, The True History of the Elephant Man. London: Allison and Busby (distributed by Schocken Books in the U.S.), 1980. 194pp. \$9.95. The extraordinary story of Joseph Merrick, the most famous "professional freak" in history. Viewed as a "monster" by contemporaries, Merrick's human survival in the face of his gigantic disorders is a marvelous wonder that should remind us of the fantastic character of the world even without any paranormal elements present.

Irwin, H.J., Psi and the Mind: An Information Processing Approach. Metuchen, N.J.: Scarecrow Press. 181pp. \$9.00. A significant work seeking to make sense of the data of parapsychology in terms of information theory.

Kline, Morris, Mathematics: The Loss of Certainty. N.Y.: Oxford University Press, 1980. 366pp. \$19.95. An important history of the recent revolutions within mathematics but of special interest for ZS readers for its implications about knowledge in general. Those who still cling to a rigid and now old-fashioned positivism would do well to read this book.

Laguerre, Michael S., Voodoo Heritage. Beverly Hills, Cal.: Sage Publications, 1980. 231 pp. \$8.95 paperback. Volume 98 in the Sage Library of Social Research, this important ethnography systematically presents--for the first time--the oral traditions of Voodoo songs and rituals. Recommended.

Langford, David, editor and commentator, "An Account of a Meeting with Denizens of Another World," 1871, by William Robert Loosley. N.Y.: St. Martin's Press, 1980. 96pp. \$7.95. An examination of a remarkable early document --very likely a hoax--claiming extraterrestrial contact. Should be of special interest to ufologists, and, if authentic (and much suggests it is not), a fascinating tale.

Laycock, Donald C., The Complete Enochian Dictionary: A Dictionary of the Angelic Language as Revealed to Dr. John Dee and Edward Kelly. London: Askin Publishers, 1978. 272 pp. No price indicated. Many have claimed to "translate" the Enochian language allegedly spoken by the angels. This is certainly the most comprehensive such work available.

LeBlond, Paul H., and John Silbert, Observations of Large Unidentified Marine Animals in British Columbia and Adjacent Waters. Vancouver: University of British Columbia, Institute of Oceanography, Manuscript Report No. 28, June 1973. 67pp. No price indicated. An important and careful analysis indispensable to anyone interested in the mystery of these reported sightings. Highly recommended.

Mackal, Roy P., Searching for Hidden Animals: An Inquiry into Zoological Mysteries. Garden City, N.Y.: Doubleday, 1980. 294pp. \$12.95. An important new work in cryptozoology that serves as an excellent introduction to the field. Recommended.

Merten, Clement, The Artist's Airbrush Manual. North Pomfret, Vt.: David & Charles, 1980. 80pp. \$25.00. A technical manual on airbrushing for the commercial artist which some readers of ZS might find of special interest for its discussions of photographic retouching, negative retouching, and special effects. Given the frequent presence of "doctored" photographs used as evidence of the extraordinary, this "how-to" book is revealing and instructive.

- McClure, Kevin and Sue, Stars and Rumours of Stars: Reports of the Paranormal in the Welsh Religious Revival, 1904-5. Privately published (8 Scotland Road; Little Bowden; Market Hartborough; Leics., England), 1980. 37pp. \$3.00, paperback. A well researched and very useful little study of the strange alleged lights phenomena reported. Of particular interest for UFO historians.
- Neher, Andrew, The Psychology of Transcendence. Englewood Cliffs, N.J.: Prentice Hall, 1980. 361+xx pp. \$7.95 paperback. A fair-minded and remarkably constructive though critical work that demonstrates the kind of positive but skeptical approach that ZS is happy to endorse. Highly recommended.
- Nisbett, Richard, and Lee Ross, Human Inference: Strategies and Shortcoming of Social Judgement. Englewood Cliffs, N.J.: Prentice-Hall, 1980. 334pp. \$14.95. If there is a single book that most readers of ZS should be familiar with, this is it. A very important survey in cognitive psychology whose message goes right to the heart of most of the issues discussed in ZS. Highly recommended.
- O'Flaherty, Wendy Doniger, Women, Androgynes, and Other Mythical Beasts. Chicago: University of Chicago Press, 1980. 182pp. \$27.50. A study of sexual metaphors and animal symbols used in Indian mythology. Of particular interest to historians of religion.
- Pecor, Charles J., The Craft of Magic. Englewood Cliffs, N.J.: Prentice-Hall, 1980. 272pp. \$6.95 paperback. An excellent introductory handbook for the would-be magician but also with material for those familiar with the art of conjuring. Dr. Pecor is a specialist in speech and the chapter on performance, "The Real Secret," is especially worthwhile.
- Philpotts, Beatrice, Mermaids. N.Y.: Ballantine Books, 1980. 96pp. \$9.95 paperback. A lovely, full-color collection of paintings and illustrations of mermaids with an excellent text and a good bibliography. Recommended.
- Podmore, Frank, The Newer Spiritualism. N.Y.: Arno Press, 1975. 320pp. \$18.00. A reprint of the 1910 book and part of the Perspectives in Psychical Research Series edited by Robert L. Morris. Podmore's skeptical approach is required reading for anyone interested in the early investigations into mediumship.
- Randi, James, Flim Flam: The Truth about Unicorns, Parapsychology and Other Delusions. N.Y.: Lippincott and Crowell, 1980. 340+xii pp. \$12.95. The title says it all. Randi is a "truth bringer" rather than a truth seeker. Though the book--like Joseph Rinn's conjuror's expose of psychical research before it-- is outrageously arrogant, overgeneralizing, and dogmatic in its tone, it still contains much valuable material which no one seriously interested in psi research can afford not to read. One does not have to agree with Randi to recognize the impact of much of his new evidence. If one can manage to ignore the crusading zealot and read the book for content rather than style, there is much valuable in it, particularly about psi research conducted at SRI. I hope that Randi's charges will be answered and not ignored, for they make an impressive prima facie case.
- Robinson, Lytle W., Is It True What They Say About Edgar Cayce? Seattle, Wash.: Vulcan Books, 1980. 185pp. \$9.95. A generally balanced attempt to examine the work of the "seer of Virginia Beach." Favorable to Cayce but not like the usual public relations efforts previously

available from his foundation and others with a vested interest in his career.

- Schutz, Albert L. Call Adonai: Manual of Practical Cabalah and Gestalt Mysticism. Goleta, Cal.: Quantal, 1980. (Distributed by Ross-Erikson.) 104+xvi pp. \$8.95 paperback. The title pretty much tells it all.
- Shepard, Leslie, editor, Occultism Update #2, 1980. Detroit: Gale Research, 1980. 126pp. No price indicated, paperback. This second inter-edition supplement to the Encyclopedia of Occultism and Parapsychology is most welcome not only for its new entries but its correction of errors in the earlier volumes as well as up-dating information, including addresses. Though reliance on sometimes erroneous sources results in perpetuating mistakes (e.g., the listing of Olaf Jonsson apparently relies on Brad Steiger's biography of Jonsson which suggested unqualified endorsement of Jonsson by J.B. Rhine), these will hopefully be corrected in future supplements as readers accept Mr. Shepard's invitation for corrections.
- Simmons, Marc, Witchcraft in the Southwest: Spanish and Indian Supernaturalism on the Rio Grande. Lincoln: University of Nebraska Press, 1980. 184pp. \$4.75 paperback. An excellent popular survey based on solid ethnohistorical and anthropological sources.
- Tefft, Stanton K., editor, Secrecy: A Cross-Cultural Perspective. N.Y.: Human Sciences Press, 1980. 351pp. \$19.95. Basically a sociological work but highly relevant to those interested in secret societies. Probably of special interest to ufologists, too, many of whom have contended national security concerns have been involved in the governments stance towards UFOs.
- Tromp, S.W., Biometeorology: The Impact of the Weather and Climate on Humans and Their Environment. Philadelphia: Heyden, 1980. 352pp. \$19.50. A thorough analytic and empirical survey of biometeorology, this work should prove indispensable to those concerned with such effects (including those interested in psi and dowsing).
- Venables, Rev. Hubert, editor, The Frankenstein Diaries. N.Y.: St. Martin's Press, 1980. A delightful "inside look" at the creator and creation of the legendary monster. Very well done spoof with marvelous illustrations from the notebooks of the bad doctor.
- Watkins, William Jon, The Psychic Experiment Book. Englewood Cliffs, N.J.: Prentice-Hall, 1980. 283pp. \$9.95 paperback. This book is clearly for entertainment rather than conducting serious experiments, but it has some surprisingly good features despite its indiscriminate acceptance of all sorts of psychic abilities. Good for parties.
- Welfare, Simon, and John Fairley, Arthur C. Clarke's Mysterious World. N.Y.: A&W Publishers, 1980. 217pp. \$17.95. A handsomely illustrated volume surveying a wide variety of exotic claims from sea serpents to UFOs. All in all, very well done, and unusually well balanced presentations.
- Wilson, Clifford, and John Weldon, Close Encounters; A Better Explanation. San Diego, Cal.: Master Books, 1978. 354pp. \$3.95 paperback. A fundamentalist Christian approach, but some interesting data is presented.
- Wilson, Clifford, War of the Chariots. San Diego, Cal.: Master Books, 1978. 191pp. \$2.95. A Christian archaeologist debates von Däniken. Some good points despite the theological assumptions.

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